le of Theory in Space Science

tional Research Council, Washington, DC

epared for

tional Aeronautics and Space Administration
shington, DC

83

Subtitle

The Role of Theory in Space Science

5. Report Date

6.

Theory Study Panel; Space Science Board, CPSMR

8. Performing Or

ng Organization Name and Address

nal Research Council;

ssion on Physical Sciences, Mathematics, and Resources

Constitution Avenue, NW

ngton, D.C. 20418

10. Project/Task/

11. Contract(C) o

(C) NASW 348

(G)

ring Organization Name and Address

onal Aeronautics and Space Administration

13. Type of Repo

Final

14.

mentary Notes

(Limit: 200 words) The goal of theory is to understand how the fundamental la
mistry give rise to the features of the universe. Space science start
observational science but now reflects interplay between observation a
is a means for analyzing and interpreting the data acquired from fligh
ents; it establishes the intellectual framework that gives meaning and
tions, ensuring that the financial and intellectual investments in fli
eriments are not wasted. We recommend that NASA establish independent
h programs in planetary sciences and in astrophysics similar to the so
physics theory program, which is characterized by stable, long-term su
n university departments, NASA centers, and other organizations engage
relevant to present and future space-derived data. We recommend that
s under review to fully benefit from the resulting research and to ass
r inflow of new ideas and investigators. We recommend that provision
computing needs of the theorists in the programs. We recommend that
geable theorists in mission planning activities at all levels, from th
-term scientific strategies through the planning and operation of spec

nt Analysis a. Descriptors

ifiers/Open-Ended Terms

TI Field/Group

ity Statement:

19. Security Class (This Report)

NAS NRC NAE IOM

PB84-178359

The Role of Theory in Space Science

Theory Study Panel

Space Science Board

Commission on Physical Sciences, Mathematics, and Resources

National Research Council

The Role of Theory in Space Science

Theory Study Panel
Space Science Board
Commission on Physical Sciences,
Mathematics, and Resources
National Research Council

NATIONAL ACADEMY PRESS
Washington, D.C. 1983

i.a

SPACE SCIENCE BOARD

COMMISSION ON PHYSICAL SCIENCES, MATHEMATICS, AND RESOURCES

HERBERT FRIEDMAN, National Research Council, Cochairman
ROBERT M. WHITE, University Corporation for Atmospheric Research, Cochairman
STANLEY I. AUERBACH, Oak Ridge National Laboratory
ELKAN R. BLOUT, Harvard Medical School
WILLIAM BROWDER, Princeton University
BERNARD F. BURKE, Massachusetts Institute of Technology
HERMAN CHERNOFF, Massachusetts Institute of Technology
WALTER R. ECKELMAN, Sohio Petroleum Company
JOSEPH L. FISHER, Officer of the Governor, Commonwealth of Virginia
JAMES C. FLETCHER, University of Pittsburgh
WILLIAM A. FOWLER, California Institute of Technology
GERHART FRIEDLANDER, Brookhaven National Laboratory
EDWARD A. FRIEMAN, Science Applications, Inc.
EDWARD D. GOLDBERG, Scripps Institution of Oceanography
KONRAD B. KRAUSKOPF, Stanford University
CHARLES J. MANKIN, Oklahoma Geological Survey
WALTER H. MUNK, University of California, San Diego
NORTON NELSON, New York University Medical Center
DANIEL A. OKUN, University of North Carolina
GEORGE E. PAKE, Xerox Research Center
CHARLES K. REED, National Research Council
HOWARD E. SIMMONS, JR, E.I. du Pont de Nemours & Co., Inc.
HATTEN S. YODER, JR., Carnegie Institution of Washington

RAPHAEL G. KASPER, Executive Director

Contents

1 Executive Summary

The goal of theory is to understand how the fundamental laws of physics and chemistry give rise to the features of the universe. Space science started as an almost purely observational science, but after more than two decades it reflects the normal interplay between observation and theory. In addition to contributing in their normal scientific role, theorists are involved in space science as strategists, advisors, critics, experiment team members, and interdisciplinary scientists.

Initially, theory is a means for analyzing and interpreting the data acquired from flight missions or experiments. During this time, theory is also developing in parallel with observations, establishing the intellectual framework that gives the facts meaning. A considerable fraction of the effort of theorists is spent constructing, refining, testing, and expanding this framework. If the framework is not in place and adequately refined when the field reaches the stage of detailed study, the previously rapid observational progress grinds to a halt. The field can be left inchoate, without direction -- or with many conflicting directions -- unable to make good on the previous investment of dollars and minds.

This stage had been reached in solar-terrestrial physics, and creative steps were recently taken to reverse the situation. It is too early to assess the results with certainty, but the new theory program in this field appears to be proceeding well. We find that similar steps, suitably adapted, would be of great benefit to astrophysics and planetary science. Both areas suffer from a lack of new talent, but for somewhat different reasons. A theory program with some assurance of stability is desirable in both areas. Participation by theorists in the advisory structure and in flight projects, while strong in some areas is much weaker in others, and theoretical participation is often the first to be eliminated when budgets are tightened. In view of the breadth of knowledge and experience that theorists can bring, the situation can be improved considerably.

We commend the National Aeronautics and Space Administration (NASA) for establishing support for an independent theoretical research program in solar-system plasma physics. The key characteristics of this program are its stable, long-term support for theorists in university departments, NASA centers, and other organizations engaged in research in topics relevant to present and future space-derived data.

PRINCIPAL RECOMMENDATIONS

We <u>recommend</u> that NASA establish independent theoretical research programs in planetary sciences and astrophysics, with objectives similar to those of the solar-system plasma-physics theory program.

We <u>recommend</u> that NASA take a long-range view of the research resulting from these programs and judge it on the quality of its contributions to new understanding of problems in these areas.

We <u>recommend</u> that NASA keep these programs under review to assure continuing opportunities for the inflow of new ideas and investigators.

We <u>recommend</u> that NASA's support for theory include provision for the associated computing needs of the theorists in these programs.

We <u>recommend</u> that NASA involve knowledgeable theorists in mission-planning activities at all levels, from the formulation of long-term scientific strategies through the planning and operation of specific missions.

2
Introduction

OBSERVATION AND THEORY IN SCIENCE

Scientific programs are carried out for a variety of reasons. Curiosity is a principal motive when a particular field of scientific enquiry is first opened, often when new technological developments make innovative measurements possible. At first, the phenomena are discovered and surveyed for their extent and variation. As partial understanding develops, questions arise that require specific new measurements, thus providing a new kind of motivation and guidance to the observer. In some cases, a theoretical prediction or speculation may provide the original motivation for an observational program. However, it is difficult to justify a continuing program of observations in the absence of new understanding derived from the observations.

In the absence of a prior theoretical prediction, a discovery and the subsequent data gathering frequently lead to unconstrained speculation about the nature of the phenomenon. Unless the underlying physical processes can soon be identified, an extensive collection of raw data may be developed. These data will be scrutinized in an attempt to detect relationships, which may provide a basis for empirical extrapolations having some predictive power. Often some physical characteristics of the environment can be deduced. This activity is called phenomenological interpretation. It often provides a basis for a later fundamental understanding of the phenomenon. Sometimes a phenomenological interpretation is mistakenly called a theory, but this confuses naming a phenomenon with understanding it. Nevertheless, phenomenological interpretation is often an essential step in the approach to understanding.

When a phenomenon is well enough characterized, many attempts will be made to explain it using physical processes that are reasonably well understood. Sometimes these attempts will be successful, and the resulting interpretations will withstand the test of further measurements. Sometimes more than one theory will be produced, so that further observations are required to distinguish between the theories on the basis of their differing predictions. On rare occasions no theories will withstand the test of further observation or the scrutiny of other theorists -- a fundamentally new law of physics or chemistry may be required.

Often attempts to formulate a theory will lead to inconclusive results. The approach appears promising, but some of the physical or chemical parameters in the theory are not well enough known to allow a comparison between the theory and the observations. One approach is to use "adjustable" parameters in the theory to fit the observations. Such theories serve a useful purpose, but danger lies in the tendency to forget the basis of such a theory and to push its predictions well beyond their tested limits of validity, generating false confidence that understanding has been fully achieved.

Laboratory measurements are vitally important to the achievement of understanding a phenomenon. Sometimes the required environment is too extreme to be reproducible in the laboratory, but it may be possible to calculate the needed parameter if the underlying physical and chemical theory is sufficiently solidly based.

Most of the phenomena studied in the space sciences occur in complicated environments. The test of whether the underlying processes are properly understood ofen requires a numerical model to be constructed using the basic laws of nature. We say that we understand a phenomenon to the degree that such numerical models agree with observation. Such comparisons must be carefully carried out, since approximations are usually needed in the numerical simulation. Numerical simulation is becoming an indispensable tool to the theorist in space sciences, and the provision of access to powerful computers for such exercises is essential to the progress toward understanding.

The ultimate goal of much of science is to provide a complete chain of reasoning extending from the basic laws of nature to the quantitative prediction of the behavior of the environment. When a field of science is that completely understood it becomes a tool for the engineer rather than a subject for further investigations. No part of the space sciences, with the trivial exception of the simplest aspects of celestial mechanics, approaches this ideal of understanding. In this report we discuss the progress that has been made toward theoretical understanding in several of the fields of space science.

STRATEGIES OF SYSTEMATIC DISCOVERY

The development of new technologies allowed scientific investigations in space science to be carried in many fields for which there was little prior knowledge. In the early phases of space research, a variety of direct and remote-sensing instruments was flown, and the principal objective was to find what phenomena could be detected and characterized. Later more systematic planning of measurements and selecting spacecraft payloads became necessary.

The Space Science Board's (SSB) Committee on Planetary and Lunar Exploration (COMPLEX) characterized planetary exploration as consisting of three stages: reconnaissance, exploration, and intensive study (Space Science Board, 1976). The SSB's Committee on Space Astronomy and Astrophysics recognized a similar exploration sequence as discovery, survey, and detailed study (Space Science Board, 1979). In each case a greater variety of data can be obtained from space observations than was available before. It has been suggested that preconceived ideas should be given little weight in the early stages of exploration. These early exploratory steps are supposed to lead to a preliminary understanding of some of the broadest aspects of the phenomena, with the expectation that more questions will be raised than answered.

This commonly accepted view of the exploration process oversimplifies the complex scientific interactions that are discussed above. Sometimes we think we know a lot about an object before it is studied from space, and these preconceptions play a major role in establishing priorities for scientific investigations. Occasionally a large body of theory exists, as in general relativity, and the principal objective of the space mission will be to test predictions. In any case, by the time we reach a stage of detailed study, further measurements will be sterile if they do not lead to increased theoretical understanding that is fed back into the planning process. As we shall see later in this report, a significant number of subfields in space science have reached the stage where further progress requires a closely coupled interaction between measurements and increased theoretical understanding.

THE INTERACTION BETWEEN THEORY AND OBSERVATION

In this report, we emphasize the need for a vigorous theory program in all NASA disciplines, one that incorporates not just elements tied to (and contributing to) specific flight missions but also a significant independent program, proceeding in parallel to the aggregate flight-mission program.

Theory will sometimes precede observations, developing -- sometimes in considerable detail -- concepts that are not yet directly accessible to observation. When well-developed concepts have spurred new observational programs, as frequently has happened, the resulting track record has been remarkably good. Even the most arcane of theoretical concepts seem to be mirrored in nature. The concept of the black hole is an almost inevitable consequence of the requirements imposed by a theory of gravitation. The development of the theory of evolution of massive stars led inevitably to an attempt to understand the physics of gravitational collapse and the black-hole phenomenon. The developmentof these theories long preceded the discovery of the first candidate stellar black-hole objects.

Indeed, the case of massive black holes furnishes an example of a more complicated interaction between theory and observation: many theorists and observers suspect that black holes of order 10^8 solar masses are the energy sources that power quasars. Theory gives arguments why this might be so, and the basic parameters are approximately in accord with observation. The observations have stimulated important advances in the fundamental theory of black holes. We do not yet know whether the final test of the hypothesis will come by way of striking new observations whose interpretation may rigorously require a central black hole or by way of new theoretical analyses that may show that data already gathered can be interpreted more uniquely than they are at present. Both theory and observation are now pressing vigorously forward.

When theory lags behind observation, it can be for either of two reasons. First, it may be that theorists have not adequately interpreted the data or developed the phenomenological or numerical models that make such interpretation possible. Such a situation is often characteristic of an underprioritized or underfunded program for theory that is directly tied to various flight missions.

Second, it may be that the framework of fundamental theory has not yet reached the point of being able to exploit the data. Indeed, the true importance of the data may not yet even be recognized. In this case, the discovery itself lies latent, until it can be unlocked by new and more powerful points of view. Theoretical developments, proceeding independently of any particular flight mission, can enhance the meaning of existing observations and can greatly increase the scientific returns on past missions or on future missions that are already frozen in design. This return is dependent on the size of the theoretical effort.

The disciplinary chapters of this report, which follow, are based on several earlier reports of the Space Science Board and the report of the Astronomy Survey Committee. Full citations for these reports are given in the References.

3
Status of Theory in Solar-System Plasma Physics

THE SCOPE OF SOLAR-SYSTEM PLASMA PHYSICS

The sciences we now variously call solar-terrestrial physics, space physics, and solar-system plasma physics began their development early in the history of Western thought. The ancient Greeks were puzzled by the "fire" in the upper atmosphere that we have named the aurora polaris. Galileo discovered sunspots in 1612, and in the succeeding centuries it was established that the number and size of sunspots increased and decreased with an approximately 11-year period. Moreover, there proved to be correlations between the sunspot activity, the intensity and location of the aurora, and fluctuations in the Earth's magnetic field. Until recently, neither experimental nor theoretical understanding was sufficient to provide any reason based in physics for the mysterious relationship between activity at the surface of the Sun and auroral and magnetic activity at Earth.

Twenty years of space research have clarified this relationship. Above the Earth's neutral atmosphere, the one we breathe, is another, made of electrically conducting ionized gas -- a plasma. Since the Earth's magnetic field controls the structure of this atmosphere, we call it the magnetosphere. Above the Sun's neutral atmosphere is another plasma atmosphere, the solar corona. The transport of energy from the solar surface to the corona generates a magnetized wind that blows throughout interplanetary space. The solar wind's interaction with Earth causes strong, highly variable motions within the magnetosphere and fills it with solar-wind plasma. Recently, we have begun to understand how the solar wind is related to the structure of the magnetic fields in the corona and to the processes generating these fields in the solar interior. Moreover, we have realized that solar-wind-generated magnetospheric activity drives strong winds in the Earth's upper atmosphere. Thus, whereas once we knew only that auroras intensify when there are sunspots, we now perceive a chain of interaction that links events at the Sun's surface to the solar wind and, hence, to the magnetosphere and atmosphere, only one of whose consequences is the aurora. This interaction chain may extend deeper into the atmosphere. Whereas 10 years ago it was generally believed that significant effects of solar variability penetrate only as far as the upper atmosphere, some scientists now suggest that they also reach the lower atmosphere and so affect weather and climate in ways not yet understood.

The discovery of the Earth's radiation (Van Allen) belts in 1958, and of the solar wind in 1960, made it clear that the exploration and future understanding of the Sun's and Earth's space environment would be couched in part in terms of plasma physics -- a discipline of classical physics that had lain relatively dormant until called forth by human need and curiosity. Since the 1960s plasma physics has developed in two separate but parallel directions. Controlled thermonuclear fusion research seeks a source of clean energy that can last for a time comparable with the present age of the Earth; the main obstacle to achieving controlled fusion lies in our imperfect understanding not of nuclear physics but of plasma physics. Solar-system space research seeks useful comprehension of nature's processes on a global, indeed a solar-system, scale, in recognition of man's intimate and sensitive dependence on his environment. It is both symbolically and substantially significant that the same discipline -- plasma physics -- defines a basic language used in both fusion and space research.

Plasma physics is the study of partially or fully ionized electrically conducting gases. It is one of the more advanced disciplines of classical physics, drawing its methodology from such diverse fields as statistical mechanics, classical electricity and magnetism, hydrodynamics, and atomic physics. The broad discipline of plasma physics may be further subdivided into three main areas: laboratory plasma physics, plasma astrophysics, and solar-system plasma physics. Solar-system plasma physics is the study of plasma phenomena throughout the solar system, extending from the solar photosphere to the outer boundary of the heliosphere (the cavity formed in the interstellar medium by the solar wind). The solar wind interacts significantly with each of the planets in the solar system. When a planet has a sufficiently strong intrinsic magnetic field, this interaction forms a magnetosphere. Either the solar-wind-created magnetosphere or the solar wind itself interacts with the ionospheres and upper atmospheres of each of the planets. The solar wind also interacts with comets, asteroids, and cosmic rays. All of these phenomena, together with those occurring on the Sun and in the solar corona, such as solar flares and prominences, are part of solar-system plasma physics.

Because solar-system plasma physics takes place in the natural environment rather than in the laboratory, purely plasma-physical phenomena cannot always be neatly isolated. For this reason, solar-system plasma physics has significant areas of overlap with other scientific disciplines, such as plasma and high-energy astrophysics and many branches of geophysics. Nowhere is this clearer than with the chain of solar-terrestrial interactions described above. Here upper-atmospheric physics, which has its intellectual roots in classical radiative-transfer physics, atomic physics, chemistry, and gas dynamics, must be joined with plasma physics to understand the interaction between the magnetosphere, ionosphere, and upper atmosphere. To investigate possible extensions of this interaction chain deeper into the atmosphere, this intellectual network will have to include meteorology and climatology.

Space observations are bringing to a close a long preliminary phase of phenomenological research in solar-terrestrial physics. The traditional subdisciplines that make up solar-terrestrial physics -- solar, heliospheric, magnetospheric, ionospheric, and atmospheric physics -- evolved from distinct origins along paths that are converging in the space era. As each moved beyond primitive phenomenology, it demanded increasingly quantitative measurements and theories. Each required close integration of different measurements to assemble complete diagnostic information about events observed by individual spacecraft. Each has now developed sophisticated ways to convert data into knowledge at this first level of integration. Probing deeper, each subdiscipline perceived its problems and regions of space to be linked to others. Multipoint data integration is an immature art whose importance grows daily. A third level of integration is needed for quantitative studies of solar activity and its effect, because the different subdisciplines have to work together. A fourth level is required to deduce, from long time series of individual events, the effects on Earth of the solar cycle and its variability. In spite of the magnitude of the task, the time is coming when relatively complete phenomenological information about the solar-terrestrial interaction chain will be available.

Solar-system plasma physics is becoming a quantitative science. Each of its subdisciplines can boast of areas where space measurements are understood quantitatively. The dynamics and structure of the Earth's radiation belts have been explained quantitatively, as has the propagation of large-scale solar-wind disturbances. The upper-atmospheric winds that are generated by magnetospheric activity have been modeled quantitatively. Withal, the level of theoretical understanding is uneven within each area and is insufficient for quantitative understanding of the complex solar-terrestrial interaction chain. Despite this fact, the increasingly quantitative nature of space research is now yielding useful understanding of solar-terrestrial effects on technological systems in space and on Earth. One example is understanding the effects of ionospheric variability on radio communication. Another is understanding the disruptions of electric power grids and pipeline monitoring systems and the spacecraft component failures that take place during magnetic storms. The increasingly quantitative nature of our understanding also means that solar-system space physics now makes important contributions to other disciplines, in areas such as magnetic-field-line reconnection in plasmas, which is thought responsible for explosive energy releases in solar flares, magnetospheric substorms, and fusion devices.

As our studies move out of the phenomenological phase into one of detailed quantitative investigations, and hence to the large-scale integration of knowledge required to understand the solar-terrestrial interaction chain, it is clear to us that the role of theory can only grow in importance. It has already begun to do so. Therefore, it is timely to review its current status and future prospects.

BASIC SCIENTIFIC PROBLEMS IN SOLAR-SYSTEM PLASMA PHYSICS

Two recent publications of the Space Science Board have discussed some of the key scientific problems at the forefront of solar-system plasma-physics research. Our discussion will therefore be brief and focus on the role of theory in advancing our understanding of them.

The Committee on Solar and Space Physics (Space Science Board, 1980) identified the following problems as amenable to fruitful attack in the 1980's:

Solar Physics

To understand better all the processes linking the solar interior to the corona, we need to study the following:

- The Sun's global circulation, how it reflects interior dynamics, could modify luminosity, and is related to the solar cycle;
- The interactions of solar plasma with strong magnetic fields -- active regions, sunspots, and fine-scale magnetic knots -- and how they cause the release of solar-flare energy to heliosphere;
- The solar corona's energy sources and the physics of its large-scale weak magnetic field.

Physics of the Heliosphere

To understand better the transport of energy, momentum, energetic particles, plasma, and magnetic field through interplanetary space we need to study the following:

- First and foremost, the coronal processes that govern the generation, structure, and variability of the solar wind;
- The three-dimensional properties of the solar wind and heliosphere;
- The plasma processes that regulate solar-wind transport and accelerate energetic particles throughout the heliosphere.

Magnetospheric Physics

To understand better the time-dependent interaction between the solar wind and Earth we need to study the following:

- The transport of energy, momentum, plasma, and magnetic and electric fields across the magnetopause;

- o The storage and release of energy in the Earth's magnetic tail;
- o The origin and fate of the plasma(s) within the magnetosphere;
- o How the Earth's magnetosphere, ionosphere, and atmosphere interact.

Upper-Atmospheric Physics

To understand better the entire upper atmosphere as one dynamic, radiating, and chemically active fluid, we should study the following:

- o The radiant energy balance, chemistry, and dynamics of the mesosphere and stratosphere and their interactions with atmospheric layers above and below;
- o The worldwide effects of the magnetosphere's interaction with the polar thermosphere and mesosphere and the role of electric fields in the Earth's atmosphere and space environment;
- o The effects of variable photon and energetic particle fluxes on the thermosphere and on chemically active minor constituents of the mesosphere and stratosphere.

Solar-Terrestrial Physics

To understand better the effects of the solar cycle, solar activity, and solar-wind disturbances on Earth, we need to

- o Provide to the extent possible simultaneous measurements on many links in the chain of interactions linking solar perturbations to their terrestrial response;
- o Create and test increasingly comprehensive quantitative models of these processes.

To clarify the possible solar-terrestrial influence on Earth's weather and climate, we need to

- o Determine if variations in solar luminosity and spectral irradiance sufficient to modify weather and climate exist;
- o Ascertain whether any processes involving solar and magnetospheric variability can cause measurable changes in the Earth's lower atmosphere;
- o Strengthen correlation studies of solar-terrestrial, climatological, and meteorological data.

The Study Committee on Space Plasma Physics (Space Science Board, 1978a) (the "Colgate" report) found that further comprehension of the problems identified above requires understanding certain abstract problems of plasma physics: (1) magnetic-field reconnection, (2) the interaction of turbulence with magnetic fields, (3) the behavior of large-scale flows of plasma and their interaction with each other and with magnetic and gravitational fields, (4) acceleration of energetic particles, (5) particle confinement and transport, and (6) collisionless shocks. These abstract problems are important in all branches of plasma physics.

Progress on nearly all of the research objectives listed above will require coordinated programs of theory and observation. To illustrate this point, we cite the fact that the Committee on Solar and Space Physics (Space Science Board, 1980) found that "comprehensive quantitative models are one ultimate goal of magnetospheric research and an essential step toward practical use of our knowledge"; they then constructed detailed lists of the models needed in each subdiscipline. Thus, the interpretation of data acquired in space will increasingly require quantitative comprehensive models. At the same time, the design of future experiments in solar-system space physics will need to take theoretical issues increasingly into account.

RELATION OF SOLAR-SYSTEM SPACE PHYSICS TO OTHER SCIENTIFIC DISCIPLINES

In this section, we describe the flow of knowledge and technique between solar-system space physics and other scientific disciplines. Only in this way can the role of theory be completely evaluated.

Space and laboratory experiments are complementary. They explore different ranges of dimensionless physical parameters. Space-plasma configurations usually contain a much larger number of gyroradii. In the laboratory, special plasma configurations are set up intentionally, whereas space plasmas assume spontaneous forms that are recognized only as a result of many single-point measurements. Space plasmas are free of boundary effects; laboratory plasmas are not and often suffer severely from surface contamination. Because of the differences in scale, probing a laboratory plasma disturbs it; diagnosing a space plasma usually does not. The pursuit of static equilibria is central to high-temperature laboratory plasma physics, whereas space plasma physics is concerned with large-scale time-dependent flows.

Certain problems are best studied in space: large-scale turbulent hydromagnetic flows and acceleration and transport of relativistic ions are examples. Certain problems could be more conveniently addressed in the laboratory, e.g., parametric studies of the effects of geometry and plasma properties on reconnection. Theory should make the results of either laboratory or space experiments available for the benefit of the whole field of plasma physics.

Twenty years of space research have taught us that the physical processes that we study of the Sun, in the solar wind, and of the Earth, occur throughout the solar system. We now study the interactions between the planets (and comets) and the solar wind in mutually reinforcing ways. Our experience with the Earth's magnetosphere was essential first to discuss the recent Pioneer and Voyager studies of Jupiter's magnetosphere and then to understand their results. It is important to note that much of the solar-terrestrial community also works actively in planetary research, so that advances in one area are rapidly communicated and applied to the other.

Because the planets and their satellites have different masses, rotation periods, surface properties, and atmospheric chemistry, dynamics, and transports, comparative atmospheric studies can help us to understand atmospheric processes in general and possibly to identify terrestrial processes that might otherwise be missed. Comprehensive studies of the interaction of the solar wind with planets and comets highlight the physics pertinent to each and put solar-terrestrial interactions in a broad scientific context. The solar system has a variety of magnetospheres sufficient to make their comparative study fruitful. The comparative study of planetary magnetospheres may well be extended to astrophysical magnetospheres in the future; it is now a general objective of solar-system space physics.

The physical processes that we study in the solar system occur throughout the astrophysical universe. For example, understanding how the Sun and planets generate their magnetic fields may help to explain why magnetic fields are so prevalent throughout the universe. Stars like the Sun that have convective outer layers are inferred to have hydromagnetically driven stellar winds; more massive stars have winds driven by radiation pressure. Plasma winds may also be generated in globular clusters and galactic halos; relativistic winds have been proposed for pulsars and active galactic nuclei. Thus, the term "magnetosphere" is used not only for the Earth and planets but also for the environments of x-ray sources, pulsars, and active galactic nuclei.

The solar system offers an excellent laboratory in which large-scale processes involving flowing plasmas and magnetic fields pertinent to astrophysics can be studied in situ. Moreover, many microscopic processes that are important to astrophysics, such as those accelerating charged particles to high energy, will probably remain forever unobservable by remote techniques. However, the analogous processes occurring in solar-system plasmas can be studied in situ.

There are striking phenomenological similarities between Jupiter's magnetosphere and those of pulsars. These similarities lie both in the macroscopic description of the magnetosphere and in the very detailed analogies between the periodic pulsed radio emissions from Jupiter and from pulsars. That said, the differences in the physical parameters pertinent to Jupiter and pulsars mean that the day-by-day problems faced by researchers studying Jupiter and those studying pulsars are quite different in detail, so much so that it is difficult for people in one field to comprehend quickly results in the other, much less to make immediate practical use of them. Nonetheless, the fact that Jupiter and pulsars present similar problems of physics is an example that may give us confidence that there exists a deeper level at which the physics of planetary and astrophysical magnetospheres is fundamentally unified.

The unifying thread linking both subjects is the common set of problems of true intellectual significance in the areas of magnetohydrodynamics and plasma physics. We have identified some of these. For example, the problem of plasma flows, relativistic or nonrelativistc, expanding from the complex magnetic fields of a condensed central body is common to solar- and stellar-wind physics, Jovian magnetospheric physics, pulsar physics, and the physics of extragalactic radio sources. Turbulent transport across magnetic fields is a problem common to every plasma, whether in the laboratory, in the solar system, or in astrophysics. We deal especially with those turbulent transport processes that change the magnetic configurations in which they occur. Magnetic-field-line reconnection is essential to understanding energetic-particle acceleration and the range of allowable magnetohydrodynamic configurations possible for every space and astrophysical magnetosphere. Field-aligned currents that link the exterior magnetosphere to its central body are common to all magnetospheres. The field-aligned current regions in planetary and pulsar magnetospheres give rise to a strikingly similar set of physical questions relating to the acceleration of energetic particles and the production of intense, coherent, highly structured radio bursts.

How then are we to proceed? We must start by recognizing the flow of information between our subjects. We start with fundamental information that comes to us from laboratory physics -- plasma physics, atomic physics, nuclear physics, and quantum electrodynamics. In the case of plasma physics, laboratory experimentation gives important assurances that certain basic processes, such as those involved with strong plasma turbulence, do in fact occur. Yet laboratory experiments cannot produce the range of parameters pertinent to solar-system plasma physics or plasma astrophysics. The in situ measurements of solar-system plasmas are the only ones that we can make on plasmas of astrophysical scale. Their completeness and accuracy make it possible to achieve detailed physical understanding of our general problems as they are manifested in the solar-system context. Just as laboratory physics provides ground truth for solar-system plasma physics, so solar-system plasma physics provides a kind of ground truth for astrophysical plasma physics. Astrophysics in turn provides a multiplicity of situations unusual to space physics sufficient to challenge the assumptions and habits of thought of those working in solar-system plasmas.

RECOMMENDATIONS CONCERNING THEORY IN SOLAR-SYSTEM PLASMA PHYSICS

The Panel on Solar System Magnetohydrodynamics, as part of the Colgate report (Space Science Board, 1978a), evaluated the 1978 status of the plasma component of solar-terrestrial theory as follows:

> The current level of theoretical effort is inadequate throughout solar-system plasma physics. The degree of inadequacy varies from subject to subject, but inadequacy exists across the board. Its level of theoretical effort is inadequate relative to other neighboring branches of physics, relative to the sophisticated data currently being collected, and is insufficient for its scientific planning, as well as increasing its interaction with other scientific disciplines. Several unresolved theoretical problems impede its scientific progress as effectively as experimental shortcomings. Its present theoretical community is probably insufficient to cope with forcseeable future demands placed on it.

In response to evaluations such as these, the Study Panel on Space Plasma Physics (Space Science Board, 1978a) made, as its principal recommendation:

> The theoretical component of the space-plasma-physics effort needs to be strengthened by increased support and, most particularly, by encouraging theory to play a central role in the planned development of the field.

The Colgate report did not address the role of theoretical atmospheric physics. In 1980 the Committee on Solar and Space Physics made the following evaluation (Space Science Board, 1980):

> The situation in theoretical upper-atmospheric physics is a happier one. For example, the need for comprehensive modeling was attended to earlier than in space-plasma physics. Nonetheless, while theoretical upper-atmospheric physics is adequate for its present tasks, its highly significant future opportunities . . . justify a strengthened theoretical effort.

THE SOLAR-TERRESTRIAL THEORY PROGRAM

Responding to the Colgate report, NASA's Solar Terrestrial Division included in its 1980 budget a line item of $2.5

million to be used to support research in solar-terrestrial theory. At about the same time NASA formed a committee, consisting predominantly of theorists in solar-terrestrial physics and headed by Robert L. Carovillano of Boston College, to recommend ways of implementing the solar-terrestrial theory program (STTP).

On July 6, 1980, A Space Science Notice (SSN) was distributed to the community, announcing the establishment of the STTP and inviting proposals for participation. In the SSN both scientific and programmatic goals were promulgated. The scientific goal of the STTP is to support substantial theoretical efforts in broad areas of solar-terrestrial research with emphasis on plasma physics. Programmatically the STTP promotes the establishment and growth of stable groups of theorists, consistent with the belief that such developments are conducive to the health and growth of the area as well as to its scientific productivity. These goals are consistent with committee recommendations to NASA.

In response to the SSN, 51 proposals were received by NASA from over 200 principal and co-investigators from the university, government, and industrial communities. The proposals were reviewed by a peer review panel, appointed by NASA's Associate Administrator for Space Science and organized as an ad hoc subcommittee of the Space Science Steering Committee. This panel evaluated the proposed research and programs using criteria developed from the goals of the STTP as stated in the SSN.

On the basis of peer evaluations, consideration of balance among the subdisciplines of solar-terrestrial physics, and funding constraints, 13 proposals were selected by NASA to constitute the STTP in its initial phase. The successful proposers represent 10 universities, 3 government or national centers, and 1 nonprofit corporate laboratory. (One proposal was submitted jointly by 2 universities.) The scientific problems being treated cover a broad range of those in solar-terrestrial physics and specifically include theoretical studies of (proceeding from Sun to Earth)

- Processes important to solar structure and activity, including opacity modifications in the interior by plasma oscillations, structure and stability of force-free magnetic fields, and magnetic reconnection;
- Hydrodynamic flows in the convection zone of the Sun;

- o Emergence of magnetic fields from the solar interior and their turbulent interactions with surface flows;
- o Heating of coronal loops by wave, current, and buoyancy processes and the EUV radiation spectra of resulting ions;
- o Particle and wave interactions in solar flares and the production of hard x rays and microwaves;
- o Evolution of the structure of Types II and III solar radio bursts and radio emissions from Jupiter resulting from nonlinear plasma wave interactions;
- o Dynamics of the solar wind, including its initial acceleration, heating of minor ions, regulation of its heat flow and temperature anisotropy, and its terminal interaction at the heliopause;
- o Acceleration of energetic particles by shock waves, co-rotating streams, and planetary magnetospheres; the resulting ionic distributions and radiation signatures; and the subsequent confinement and propagation of such particles;
- o Microstructures of quasi-perpendicular and quasi-parallel collisionless shocks, their relationships to the gross structure of the Earth's bow shock, and the generation of waves and accelerated particles away from the shock;
- o Nonlinear plasma tearing mode and its role in the dynamics of the Earth's magnetotail;
- o Global magnetohydrodynamic structure of the Earth's magnetosphere;
- o Plasma and energy transport into the magnetosphere driven by turbulence associated with density gradients at the magnetopause;
- o Nonlinear evolution of kinetic plasma instabilities in the Earth's magnetosphere, including electron and ion cyclotron waves and their concommitant plasma heating and transport;
- o Processes that might produce Earth's electromagnetic continuum radiation;
- o Kelvin-Helmholtz instability of a layer of finite thickness and its bearing on magnetopause and plasmapause structure;
- o Kinetic Alfvén waves propagating from the magnetosphere to the ionosphere;
- o Interhemispheric transport of plasma and its effect on the conjugate ionospheres;
- o Structure of electric double layers and their relationship to auroral arcs;

- o Stability of Earth's polar wind;
- o Electromagnetic coupling of the magnetosphere-ionosphere-upper atmosphere system at high latitudes;
- o Low-beta convection in the Earth's ionosphere.

By design the STTP has a strong plasma-physics flavor, although there are also substantial activities in the areas of hydrodynamics, atomic physics, and cosmic-ray physics.

Several of the investigators have experience in plasma physics acquired in the controlled fusion and other laboratory plasma programs and have come into space physics through the STTP. While they are not the majority of researchers in the program, they bring to the STTP knowledge of the wealth of theoretical plasma physics that has developed over more than 25 years of laboratory plasma-physics research. These researchers also bring computer models that in many instances can be adapted readily to solar-terrestrial problems. Modeling is thus an integral element of STIP, although it is balanced by a substantial effort in analytical theory.

The sizes of individual groups range from 4 to 11. A given group may be working on several (related) problems concurrently. At present approximately 100 persons, representing approximately 45 man-years of effort, are being supported by the STTP.

To enhance continuity of research, funding is provided through continuing grants, i.e., full proposal submission and review occurs only at 3-year intervals. In intervening years renewal is based on a letter proposal that includes a brief summary of the prior year's achievement, the next year's plans, and an updated budget. The community will be notified again of new opportunities that may arise from program expansion and/or turnover.

NASA conducts annual internal reviews of the STTP. As perceived necessary or advantageous, the STTP organizes or supports workshops, conferences, and other activities in solar-terrestrial theory and related topics. A major external review is planned for 1982 before the end of the first 3-year period and in time to evaluate its results before renewal proposals are requested. This is expected to be the first juncture for possible program turnover.

4
Status of Theory in Astrophysics

INTRODUCTION

Space missions provide unique means with which to carry out scientific investigations of aspects of our universe that are otherwise inaccessible. Such investigations advance our knowledge of natural phenomena and illuminate our current understanding, from the Sun to distant, exotic objects, and even to the universe itself. At the same time, space missions are discrete opportunities that must be used wisely to investigate major issues, and therefore care is required in choosing them.

Theory provides a framework within which observations can be related to each other and to our present understanding. Theory can unify otherwise disparate observations and integrate the study of similar phenomena in widely different contexts. Theory also plays a key role in defining the significance of previous observations and possible new measurements, a process that leads to new ideas and new motivations for future missions. Theory also helps to determine the precision necessary for observations to have a significant impact on our understanding of natural phenomena by, at the very least, differentiating among our various preconceptions. Because of these roles, a strong and vital theory program is essential to optimal planning and exploitation of space-science missions.

The nature of space exploration in astronomy is changing. Earlier, discoveries occurred by looking and seeing what was there. This was repeated as each new electromagnetic wavelength band became accessible via space observations. But these wavelength windows have now been opened, and in many areas of astronomy space missions have changed from surveys to specific observations using particular analytical instrumentation. The number of ways in which objects and phenomena can be studied is great. Most of these cannot be pursued, and difficult choices must therefore be made. Here input from theory is critical. The new sophistication and power of space instruments requires a much more sophisticated knowledge of the physics of astronomical objects in order to be used effectively. Here again theory must provide this more detailed knowledge. The explosion of astronomical data that will come from space missions under way and planned demands a strong theoretical framework. Otherwise, the new data cannot be assimilated and the field of astronomy becomes inchoate. These changes create an increasing need for theoretical research during the coming decade.

IMPACT OF THEORY

Current major themes in astrophysics exploration include cosmology; formation and evolution of galaxies and galaxy clusters; stellar evolution, especially the births and deaths of stars; properties of compact objects (degenerate dwarfs, neutron stars, and black holes); and astrophysical plasmas, such as stellar coronas, winds, and mass loss, and the interstellar and intergalactic media. Below we give a few examples of the ways in which theory has been, is, and will be essential to exploiting space observations in each of these areas.

Cosmology

Origin of the Universe. The "big-bang" model of the origin of the universe has been of enormous importance in interpreting observations and in formulating new ones. It led to an understanding of the first observations of the microwave background and has motivated observational determinations of helium and deuterium abundances, which provide evidence of conditions during the earliest history of the universe, as well as deep-sky studies of the distributions of quasars and galaxies, from which the expansion rate of the universe may be determined. The big-bang model is favored because it explains each of these observations in a simple and natural way; however, the observational support for the model is limited, and theorists therefore have a special obligation to look for new ways in which it can be tested. The Cosmic Background Explorer will provide stringent measurements of the spectral shape and isotropy of the microwave background. Interpretation of the results requires theoretical modeling of the effect on the background due to inverse Compton scattering by distant gas. The Space Telescope and the Advanced X-Ray Astronomy Facility will probe helium and deuterium abundances, and the abundances of heavy elements, to higher red shifts and therefore earlier epochs. Theoretical modeling of the helium and heavy-element abundances to be expected in intracluster gas due to early star formation and injection from galaxies are needed to interpret the forthcoming data.

Fluctuations in the Early Universe. Within the standard big-bang model, the diversity of structure in the present universe is a consequence of the growth of fluctuations in the early universe. Among the unanswered questions within this framework are, can the mass spectrum of required fluctuations be understood, how did galaxies evolve after they condensed out of the expanding medium, and what is the origin of the violent events in the nuclei of some galaxies? The Cosmic Background Explorer will provide detailed data on the angular and frequency character of the cosmic microwave radiation. Theoretical work is required to convert these data into a picture of conditions at the epoch of decoupling of matter and radiation. Newly born galaxies are expected to be brightest in the infrared because of their large red shifts. This fact and their faintness may make them accessible only to space observations with instruments such as the Shuttle Infrared Telescope Facility. Among the challenges faced by theory is to develop detailed models for the early evolution of galaxies that can be compared with the forthcoming observations.

Massive Neutrinos. Recently, claimed experimental evidence for finite-mass neutrinos has stimulated a flurry of theoretical activity since the mass inferred would imply that primordial neutrinos contribute a significant, perhaps dominant, fraction of the present mass of the universe. Important theoretical questions that need to be pursued include how these neutrinos would cluster around galaxies during their formation, and how we can determine observationally the distribution of massive neutrinos today.

Supernovae as "Standard Candles." Reliable determinations of the expansion and deceleration rates of the universe must be based on the use of physically well-understood objects for measuring distances. Supernovae are one possible kind of "standard candle." Spectroscopic observations of supernovae during the first month after maximum luminosity, using the Space Telescope, can determine the distance to the supernova through the time dependence of the continuum spectrum and the Doppler shift of the spectral lines. Theoretical calculations of supernova atmospheres are needed that can be compared with the frequency and time dependence of the observed spectrum.

Evolution of Quasars, Galaxies, and Galaxy Clusters

Galaxies as "Standard Candles." Understanding the evolution of galaxies and clusters of galaxies is essential to using these objects as "standard candles" for measuring distances, and thereby establishing the expansion and deceleration rates of the universe. Theorists have modeled the evolution of galaxies by considering the effects of an epoch of massive star formation at early times, by calculating the mass spectrum of stars and the rates of star formation and death, and by treating the nuclear processing of matter that occurs within stars. From these various factors are derived the variations with time of the luminosity, colors, and element abundances. The Space Telescope and the Advanced X-Ray Astronomy Facility will detect these variations at earlier epochs than have been accessible before. Theoretical studies of galaxy formation and of the early evolution of galaxies are essential to interpreting the data that will be returned from these missions and to successfully separating the intrinsic variations from those due to expansion of the universe.

Supermassive Black Holes. One of the most pressing issues to be addressed by the Space Telescope is whether active galactic nuclei and quasars are powered by accreting supermassive black holes The observational data will be in the form of brightness variations and velocity dispersions as a function of distance from the center of the galaxy. Both will come primarily from observations carried out using the Space Telescope, but the Shuttle Infrared Telescope Facility may be the only means of garnering this information for very distant active galactic nuclei and quasars because of their large red shift. These data can be used to infer the mass of the central object, if theoretical calculations of the dynamics of stars orbiting a supermassive black hole are available for comparison. In the case of active galactic nuclei, a basic question is whether supermassive black holes that accrete stars, gas, or both exhibit a unique signature that can be identified through such observations. More theoretical studies are required in order to elucidate the expected observational differences between supermassive black-hole models and other possibilities, such as a dense cluster of stars. In the case of quasars, much more modeling of accretion disks, of black-hole magnetospheres, and of the dynamics of stellar systems in the vicinity of a supermassive black hole are badly needed to test the black-hole model. In both cases, computational facilities capable of handling large n-body calculations will be essential.

Jets. One of the ubiquitous features of violent phenomena in the universe appears to be the production of relativistic plasma beams or jets. Such structures are encountered in the fascinating galactic object, SS433, in double radio sources associated with distant galaxies, in quasars, and possibly even in star formation. However, these phenomena are poorly understood. New information about them will be forthcoming from the Space Telescope, the Advanced X-Ray Astronomy Facility, and eventually from space-based very-long-baseline interferometry. In order to understand these phenomena, and use that understanding to address questions about the nature of active galaxies and quasars and the way in which galaxies themselves are formed, it is essential to carry out studies of relativistic hydrodynamics, magnetohydrodynamics, and electrodynamics, including problems of particle and fluid acceleration, interaction with relativistic gravity, shock stability, and extensive modeling of jetlike flows.

Dynamics of Galaxies. Recent theoretical ideas about the way in which galaxies interact with one another have had an important impact on observations. These ideas include the existence of galactic halos, consisting of a so-called stellar population III or some other faint form of matter such as massive neutrinos. The determination of their size and mass and the nature of their constituents are among the most pressing issues in astronomy. The Space Telescope, the Shuttle Infrared Telescope Facility, and perhaps some kind of large deployable reflector will search for faint optical and infrared light from halos composed of low-mass stars. The proposed Advanced X-Ray Astronomy Facility will search for the x-ray emission produced by the accretion of halo gas onto any black holes. Many more theoretical analyses of the observational consequences of halos composed of low-mass stars, compact objects such as black holes, massive neutrinos, and other possibilities are needed. The role of interactions between galaxies, such as tidal stripping of galaxies by neighboring ones in a cluster and even the "cannibalism" of galaxies by one another, is now thought to be of great importance not only for galactic halos but for all aspects of their structure. For example, interactions between galaxies are currently receiving a great deal of attention as a possible means of producing the extended distribution of matter observed in galaxies. Observations using the proposed Advanced X-Ray Astronomy Facility promise new information about the morphology of clusters from studies of x-ray emitting intracluster gas, while the Space Telescope may provide new observations of violent galactic encounters at earlier epochs. Additional theoretical calculations of galaxy collisions and cluster formation and evolution will be needed to exploit such observations.

Stellar Evolution

Stellar Births. Considerable effort will be expended during the forthcoming decade on missions in the infrared such as the Infrared Astronomy Satellite, the Shuttle Infrared Telescope Facility, and perhaps a large deployable reflector that will explore the earliest stages of star formation and evolution. Dark interstellar clouds, in particular, will be studied with high spatial resolution and their chemical (molecular) composition and velocity fields determined. Theoretical modeling of molecular clouds, their formation, evolution, and chemistry, as well as their collapse, fragmentation, and dissipation, will be critically important both in planning these missions and in analyzing their results. Through application of two- and three-dimensional hydrodynamical codes, and from improved knowledge of important interstellar chemistry, it will be possible to construct more realistic models of collapse and fragmentation. These studies should indicate the angular resolution required in order to study regions of active star formation. Access to large (CRAY-class) computers and to VAX-class computers equipped with array processors will be necessary in order to implement these calculations. Support of laboratory studies of molecular line spectra should allow identifications and provide an understanding of transitions arising from molecules important in probing the structure and heat balance in cloud complexes.

Stellar Deaths. A firm understanding of supernovae is a long-standing and important goal in astrophysics that continues elusive. Crucial new information has become available over the past decade from elementary-particle physics in the form of the Weinberg-Salam theory of weak interactions. Combined with this have been new insights by theorists in astrophysics, such as the recognition that the fermion nature of neutrinos must be taken fully into account. Also important have been new computer calculations of the late stages of stellar evolution and the onset of stellar collapse. These theoretical studies should eventually provide supernova radioactivities and line intensities that can be compared with data from the Gamma Ray Observatory, ratios of cosmic-ray isotopes injected into the galaxy by supernovae that can be tested by an advanced interplanetary explorer, and abundances in supernova remnants and the character of supernova shock waves in the interstellar medium that can be compared with observations from the proposed Advanced X-Ray Astronomy Facility. Such efforts need sustained support and access to large mainframe computing facilities capable of handling the complex calculations required to represent the rich physics involved in supernova explosions. Also needed are further studies of: the properties of hot dense matter (particularly near and above nuclear matter density); shock propagation and neutrino emission; the effects of rotation, magnetic fields, and mixing. These types of calculations will require the largest computing facilities.

Supernova Light Curves and Infrared Lines. Numerical models of radioactivity-powered Type I supernovae give light curves and optical spectra in excellent agreement with observations. These same models predict a flux of infrared iron line emission that could be detected by the Shuttle Infrared Telescope Facility from as far away as the Virgo cluster of galaxies and could be studied in detail by a large deployable reflector. More realistic numerical models of supernova atmospheres should be carried out that include the effects of velocity and composition gradients. Also needed are studies of the red-shifted light curves and emission lines to be expected from the explosions of massive stars formed at very early times in the evolution of the universe.

Properties of Compact Objects

Gamma-Ray Bursts. One of the most important discoveries in gamma-ray astronomy has been that of cosmic gamma-ray bursts. Considerable observational data have accumulated about them over the last 15 years, and their study is a mission goal for the Gamma Ray Observatory, yet our theoretical understanding of them remains primitive. The currently favored model is one in which the bursts originate from a hot, magnetically confined plasma surrounding a neutron star. Detailed (probably multidimensional) numerical calculations of radiation transport in magnetic plasmas are necessary to understand the cyclotron and pair annihilation features observed in the bursts. Theoretical modeling of both accretion and thermonuclear instabilities on magnetic neutron stars are also badly needed to test the favored model against new observations from balloon flights and from the Gamma Ray Observatory. The results of the theoretical work will aid not only in understanding currently planned observations but in specifying the needed characteristics of future ones. For example, some models predict an extended period of soft x-ray emission following the hard gamma-ray burst. If so, spectral observations of gamma-ray bursts at energies lower than the traditional lower limit of about 100 keV will be important. The required timing resolution may also be set by theory, which predicts that the intensity and the spectrum of the bursts can vary on a time scale as short as a thousandth of a second, the dynamical time scale at the surface of a neutron star. Numerical computations in one and two dimensions, possibly involving magnetohydrodynamic effects, as well as radiation transport and hydrodynamics, are needed to give a detailed picture of this temporal evolution.

Compact Galactic X-Ray Sources. With the launch of the Uhuru x-ray satellite in the early 1970s, hundreds of pointlike x-ray sources were revealed. The interpretation of these sources in terms of accretion of gas from a binary companion onto a compact object (either a white dwarf, neutron star, or black hole) relied on a large and detailed body of theoretical work on not only the physics of such compact objects but also the evolution of binary systems and the hydrodynamics of mass transfer. This work had been done throughout the 1960s independently of any planned space missions. For example, the important and exciting possibility that Cygnus X-1 is a black hole relies on knowledge of the maximum possible mass of a neutron star and therefore on our understanding of the properties of dense matter and relativistic gravity, as well as on studies of the physics of black holes. As a second example, theorists concluded that the bright pulsing x-ray sources were neutron stars from the rate of their spinup. They then predicted that the pulsing x-ray sources should all lie on a particular curve that is a function of their luminosity and pulse period. This prediction has been confirmed by later observations.

Theoretical Initiative for the X-Ray Timing Explorer. Long-duration timing observations of pulsing x-ray sources in binaries can provide important information about, for example, binary systems and their evolution, the masses and internal structure of neutron stars, and the behavior of matter at very high temperatures and in intense magnetic fields. Observations with very high time resolution may provide information about the numbers and properties of stellar black holes. In 1978, a group of theorists became concerned that no mission was currently approved or planned that could exploit the rapidly advancing theory of compact x-ray sources. A workshop organized by these theorists generated widespread support and enthusiasm throughout the observational community for such a mission and documented the scientific goals that could be met by it. NASA responded with an Announcement of Opportunity for an X-Ray Timing Explorer.

Gamma-Ray Lines. Gamma-ray lines have been observed in the spectra of a number of astronomical objects: the Sun, gamma-ray burst sources, and the galactic center. Although not yet detected, gamma-ray lines are expected from the decay of recently synthesized nuclei in our own and in other galaxies. Future missions, especially the Gamma Ray Observatory, will spend a considerable portion of their time examining these lines. Theory must aid these observations by deciding where to concentrate the effort. It has been shown, for example, that the most likely candidates for producing observable gamma-ray line fluxes due to radioactivity in nearby galaxies are Type I, but not Type II, supernovae and that the lines from such objects are likely to be quite broad. Positron production by cosmic rays, supernova radioactivities, and accretion disks around supermassive black holes also need theoretical study in order to understand the origin and possible time variability of the 511-keV line emission from the galactic center. As already mentioned, detailed numerical calculations of radiation transport in a hot, magnetically confined plasma are required to understand the cyclotron absorption and pair annihilation features observed in gamma-ray bursts. These calculations are needed not only to understand the current observations but to determine the time resolution and spectral sensitivity required for future missions.

Astrophysical Plasmas

Cosmic Rays. Studies of cosmic-ray composition have historically centered on unraveling the origin, acceleration mechanism, and life history of energetic particles from abundances observed at Earth. Theoretical models of propagation are required to extrapolate measured compositions back to the source. In the case of isotopic ratios, which are far less sensitive to propagation and chemical effects than elemental ratios, theory is useful in deciding which isotopes, and therefore what level of sensitivity, should be employed in space missions, such as the proposed advanced interplanetary explorer, as primary diagnostics of cosmic-ray origins and age. Once the measurements have been made, theory is essential to translate the results into meaningful constraints on the origin and acceleration mechanism. Is the accelerated material principally unevolved matter from the interstellar medium, or recent ejecta from a particular class of supernovae, or a mixture of the two? Theoretical studies of cosmic-ray acceleration and propagation models, plus the measurement or calculation of relevant cross sections, computer models of supernovae and nucleosynthesis, and theoretical studies of expanding supernovae interacting with the interstellar medium are required to address these issues.

Solar-Active Regions and Flares. Solar studies have and will continue to play a unique role in theoretical astrophysics, as the proximity of the Sun permits observations at all wavelengths with sufficient spatial resolution to study the important physical processes on their own spatial scales. In particular, one can study directly the interaction of magnetic, velocity, and radiation fields under both steady-state and highly transient (flaring) conditions. Important physical processes that can be studied in detail on the Sun but also occur widely in astrophysics include the generation and decay of magnetic fields, generation and dissipation of magnetohydrodynamic waves, a wide variety of nonradiative heating sources, high-energy particle acceleration, plasma to thermal and nonthermal energy deposition. Understanding these processes in the Sun is an essential prerequisite to applying them to understand phenomena elsewhere in astrophysics. The most energetic and rapidly varying phenomena occur in solar-active regions and, in particular, during flares. The complement of experiments on the recent Solar Maximum Mission was chosen specifically to understand physical processes occurring in flares, and the important theoretical questions to be answered included: what is the energy source of the flares, what plasma instabilities trigger the rapid heating in flares, how are the high-energy particles accelerated, and what cools the flare plasma? Further theoretical work on all of these questions is essential.

Stellar Winds and Coronas. One of the great triumphs of theoretical astrophysics was the prediction by E. N. Parker in 1958, prior to the first direct detection of the solar wind by the Lunik III and Explorer 10 spacecraft, that the Sun is losing mass in a wind. Subsequent solar observations have led to refinements of this theory and to the realization that Alfvén waves play an important role in the acceleration processes. The Einstein x-ray observatory and the International Ultraviolet Explorer have shown that most stars have hot coronas but that only a limited range of stars similar to the Sun have winds accelerated by the Parker-type mechanism. Many stars have mass loss rates that are a million, or even one hundred million, times that of the Sun. Such mass loss rates are large enough to change the evolution of the star and to enrich the interstellar medium with heavy elements. The mechanisms by which these intense winds are accelerated are in dispute; radiation pressure, radiative instabilities, and coronal-type winds are plausible contenders for the hot stars, while Alfvén waves, periodic shock waves, and radiation pressure on dust grains are contenders for cool stars. Future ultraviolet and x-ray observations by the Space Telescope, the proposed Extreme Ultraviolet Explorer, and the proposed Advanced X-Ray Astrophysics Facility will help to determine which acceleration mechanisms are important for different types of stars. But additional theoretical work is badly needed on the various acceleration mechanisms mentioned above in order to provide a framework within which the observations can be interpreted.

Relativistic Gravity

Black-Hole Physics. Discussions above have emphasized the possible roles of black holes in quasars, galactic nuclei, and compact x-ray sources. It may turn out that we do not yet understand all the fundamental physics governing black holes well enough to carry out astrophysical model building adequately. For example, only in 1977-1981, in response to the discovery of jets in galactic nuclei and quasars, did relativity theorists decipher the physics governing the interaction between black holes and magnetic fields -- physics that may be responsible for jet production. Thus, observations have triggered research in general relativity, which in turn has had an impact on astrophysical theory in ways that will affect future observations and data analysis -- and this may well happen again in the future.

Dynamical Gravity and Gravitational Radiation. Doppler tracking of spacecraft is being used to search for gravitational radiation from the births of supermassive black holes and collisions between supermassive holes out to the Hubble distance. The Space Science Board has recommended a feasibility study of an optical heterodyne system that might be developed and used in the 1990s with gravity-wave sensitivity 7 orders of magnitude greater than the current Doppler system. This observational program may reveal to us a new region of space-time in which gravity (space-time curvature) is ultrastrong and highly dynamical. The relevant computer calculations are numerical solutions of the Einstein field equations of general relativity -- calculations that in vacuum (e.g., black-hole collisions) have no uncertainty in their input physics. The technology of such calculations with axisymmetry (two space dimensions and one time) is now being developed vigorously, but non-symmetric (three space and one time) calculations have yet to be attempted. Such calculations, which may require 5 years or more to perfect, are a prerequisite to the understanding and interpretation of future gravitational-wave data (if any). The development of such calculations should not await a successful outcome of the gravity-wave search.

Theoretical Interpretation of Experimental Gravity. In the past NASA has buttressed its successful program in experimental tests of general relativity by a low-level theoretical program (see Space Science Board, 1981, page 51). This program has proved sufficiently useful that it should be continued in the future at the same level as in the past.

CURRENT STATUS OF ASTROPHYSICAL THEORY

In order for theory to realize its important and proper role, two elements must exist: a strong and independent program of theoretical studies and participation and involvement of theorists at all levels of mission planning. The first requires a range of theoretical activities, from the analysis of data through basic and fundamental theoretical studies not associated with particular missions or mission objectives. Such a range of activity is necessary for a healthy and vibrant theoretical community, capable of supporting the present directions of space-science investigation but also able to support new and unexpected directions. The participation of theorists is also needed at all levels of mission planning, from the development of space-science strategies, to the definition of individual missions, through mission operations; this is addressed explicitly in Chapter 7.

Despite the pivotal role played by theory in astrophysics, its current status, when measured against the first requirement discussed above, is poor. Few strong theoretical groups now exist, and there is no coherent NASA program of support for individual theorists or for theoretical groups in astrophysics. The National Science Foundation (NSF) provides some theory support, but it is inadequate to meet the needs of the space missions now under way and planned. Currently, theoretical astrophysics research (excluding solar-system physics), is funded by NASA at the level of approximately $1.1 million per year (fiscal year 1982) and by NSF at a level of approximately $2.5 million per year (fiscal year 1982). These programs consist of approximately 30 university grants by NASA and 60 by NSF, each of approximately $30,000. NASA also supports some interpretive theory related to specific missions; we have not been able to make a reliable estimate of the level of this support. The total NASA support of theoretical astrophysics, including the staff theorists at the national centers, represents approximately 0.8 percent of the NASA budget for astrophysical science (about $300 million in fiscal year 1982). The total NSF support of theoretical astrophysics including staff theorists at the centers represents approximately 5 percent of the NSF annual astronomy budget (about $60 million in fiscal year 1982).

In our opinion, the support of theoretical astrophysics research at the universities has declined to a dangerously low level. In the current situation, the vigor of the few strong theory groups is threatened. Excellent theorists are unable to obtain funding or face great delays in obtaining funding. Many theorists are in their second, and even third, postdoctoral appointments. As a result, many trained young theorists are having to leave astrophysics, and the number of bright young graduate students entering astrophysics (the lifeblood of the future) is declining markedly. This comes at a time when changes in the nature of space exploration in many areas of astronomy require more, not less, theory and when the recent and coming explosion of data from space missions creates an increasing need for theoretical research. Below we give three examples of ways in which NASA missions could have been improved with more theoretical support.

Pulse Profiles of Neutron-Star X-Ray Sources. Detailed observations of the x-ray pulse profiles produced by accreting magnetic neutron stars were obtained by the Uhuru and, especially, the SAS-3 satellites. Spectra of these sources have been measured by OSO-8 and, in the case of

Her X-1, by more recent rocket flights. Over more than a decade, these observations have been largely unexploited owing to a lack of theoretical calculations with which they could be compared. First needed have been calculations of fundamental processes, such as Coulomb and electron scattering, and cyclotron absorption, in high-temperature plasmas in intense magnetic fields. Second, detailed numerical modeling of the x-ray emission region, incorporating both radiation transfer and hydrodynamics, are essential. Recently, a decision was made by the Max Planck Institute in Garching to fund such studies. Among workers hired there have been several from the United States. These theorists have launched a large-scale and quite successful attack on both parts of the problem. As a result, for the first time meaningful comparisons are being made between the observations and theory. These comparisons have provided information about the physical conditions in the x-ray emission region, have pointed out the importance of coupling between the normal modes of the magnetic plasma, and have provided evidence that the observed cyclotron feature in Her X-1 is due to absorption and not emission. With adequate support, this work could have been done in the United States 10 years ago.

Evolution of Neutron Stars. Recent observations of supernova remnants and nearby pulsars using the Einstein x-ray observatory have provided important detections and stringent upper limits on the soft x-ray emission from cooling neutron stars. Theoretical calculations have played a crucial role in providing benchmarks against which the observations can be contrasted and have enabled important implications to be drawn from the observations. Among these are evidence that some supernova remnants do not contain neutron stars, and thus some supernova do not produce neutron stars, or that the neutron stars have cooled more rapidly than allowed by the "standard" cooling picture. If the latter is true, it suggests that the interiors of neutron stars contain a condensed pion phase or, possibly, free quarks; this provides important information about the properties of dense matter. However, the observations cannot be fully exploited because of a lack of theoretical knowledge about various aspects of neutron star-evolution. These include an almost total lack of knowledge and study of important energy-transport processes in strong magnetic fields, of the properties of matter in strong magnetic fields as encountered in the surface layer of strongly magnetic neutron stars, and of the emissivity and atmospheric structure of such stars. No obstacle stands in the way of a concerted theoretical attack on these problems, except a lack of support.

Clusters of Galaxies. Some of the most fascinating and significant revelations obtained from observations made with the Einstein x-ray observatory have been the rich and varied structures of galaxy clusters. Some x-ray "photographs" have shown clusters that are roughly spherical, often with an active cD galaxy near the center; others have revealed two distinct clusters in the process of merging, while still others have shown complex, multicomponent structures. Residing in this wealth of new information are important clues about the way in which clusters, and the galaxies of which they are composed, are formed, as well as exciting information about the open or closed nature of the universe. The observations cannot currently, and will not in the near future, be fully exploited because of a lack of theoretical studies of cluster formation and dynamics.

Such situations multiply, and their negative impact on the effective utilization of space missions and the vitality of space science will continue to increase unless reversed by significant new support for theory.

5
Status of Theory in Planetary Sciences

INTRODUCTION

The fundamental goals of planetary science are to build our understanding of the origin of the solar system, to define the physical processes that produced the planets and were involved in their subsequent evolution, to elucidate the effects that continue to shape planetary environments, and to discover the conditions and processes that led to the formation of life. In the modern discipline of planetary science empirical information gathering is accomplished through the use of technically sophisticated spacecraft, together with ground-based, earth-orbital, and rocket observations, to probe and measure the properties of solar-system objects, and laboratory analyses of extraterrestrial materials. The full scientific and intellectual endeavor requires, however, deep and coordinated research efforts in the theoretical disciplines, as well as in the laboratory, in order to marshal our understanding on a firm foundation of physical principles.

Evidence today converges to support the idea that the planetary system formed simultaneously with the Sun from a collapsing cloud of interstellar gas and dust. The cloud's pre-existing angular momentum dictated the initial formation of a disk-shaped nebula from which planets accumulated into the nearly planar distribution that we see today. In this picture the differing compositions of the planets reflect the systematically varying conditions, with distance from the Sun, of temperature and pressure in the protoplanetary nebula. Some of the planets do not fit easily into such a sequence, however, and this poses puzzles to our understanding.

The larger planet-sized objects formed with sufficient sources of internal energy to fuel continuing evolutionary activity. Consequently much detailed information about their initial characters has been lost. Their masses and bulk compositions, however, carry important clues about the processes of planet formation. Beyond that, the challenge these bodies present to us is to unravel the reasons for their widely diverse evolutionary paths and to comprehend the stability and instability of planetary environments, including our own.

Some smaller objects apparently formed with insufficient resources of internal energy to fuel long-term evolution of the kind undergone by the larger planets. These bodies, including comets and asteroids, thus retain evidence of the primitive processes of solar-system formation; they are our most important source of clues for the events and conditions that produced the solar system. So far our only direct measurements of the properties of such objects come from laboratory analysis of meteorites and of a growing collection of interplanetary dust grains.

Historically, planetary science had its beginnings in early astronomy where the brightness of the nearer planets and their motions relative to the background stars made them, after the Sun and Moon, the most conspicuous objects in the sky. Prior to the space age, the orbits of the planets and the general configuration and content of the solar system had been established along with the masses and mean densities of most of the planets and some of their moons. Rough estimates of compositions were inferred from the mean densities. Except for Earth and its Moon, the surface characteristics of all solar-system objects remained obscure. In the planetary atmosphere only a few molecular species could be detected from ground-based telescopic observations.

This changed drastically with the active exploration of the solar system by American and Soviet spacecraft and with numerous technological innovations in ground-based observational technique. New and exciting data attracted scientists from many disciplines to the study of solar-system objects. Many old ideas had to be abandoned as our detailed information about the planets increased. Conclusions were more and more based on developments from fundamental principles and tested by detailed observations, and the study of the solar system became a flourishing science. Properties of planetary interiors are now inferred from models that take into account the high-pressure behavior of matter and that are constrained by the planets' externally observed properties. Surface characteristics of the terrestrial-type bodies have been determined, giving clues to the physical processes that have shaped those planets. The dynamics and chemistry of atmospheres far different from our own on Earth have been studied, posing puzzles about the behavior, formation, and evolution of planetary atmospheres. The influx of spacecraft data has been complemented by many ground-based and near-Earth observations. Study of the solar system now involves virtually every branch of science -- in sharp contrast to the situation before 1957.

The detailed information available about terrestrial-type planets allows comparisons with Earth and demands application of the extensively developed earth sciences to the planets. In return, added insight into the workings of our own earthly environment is possible, and there is a growing interchange between earth and planetary science. Our increasing knowledge of the current state of the solar system and of the objects in it reveals clues about its origin and evolution. The stellar status of the Sun inextricably links the study of the origin of the solar system with that of the formation of stars in general. Thus, our accumulating clues about the birth of the solar system influence our broader astrophysical ideas about the formation of stars.

Continued data collection, beyond an initial exploration phase, would lack direction and therefore be pointless without theoretical interpretation and the construction of models consistent with and extrapolating from those data. Theoretical ideas and models help to define the physical and chemical processes behind observed phenomena and thereby increase our understanding in terms of fundamental physical principles. The need for further measurements as well as the required accuracies emerge from such theoretical study and result in a constant interchange between theory, numerical simulation, and observation.

A few representative examples of the role that theoretical studies play in this interchange, as well as in the general development of our understanding, will be discussed below with specific reference to the areas of planetary interiors, surfaces, and atmospheres and to the overall problem of the solar system's origin. The examples are by no means exhaustive; only a few topics are chosen for illustration. Each example includes a discussion of some of the research that would advance our understanding and thereby define future observations and measurements. Finally the state of support for such research is evaluated.

PLANETARY INTERIORS

In addition to testing our grasp of the basic physical phenomena that shape the planets, modeling of planetary interiors and processes and their external manifestations provides important information about the inventory, distribution, and thermochemical state of planetary constituents. The relevant fundamental theoretical principles range from reasonably well-understood static considerations (hydrostatic equilibrium) through less well-understood microscopic material properties (flow and deformational properties, electrical and thermal conductivities) to the poorly understood macroscopic transport processes (convection, differentiation, volcanism). Theoretical calculations achieve their greatest predictive power when they are coordinated with laboratory and observational data. For example, existing experimental data concerning the equations of state and phase diagrams of cosmochemically important compounds and their mixtures at appropriate pressures and temperatures are inadequate for answering such questions as the partitioning of sulfur and oxygen in the terrestrial planets or of helium, water, and ammonia in the outer planets. As these and other data become available from currently progressing work in high-pressure physics a much improved theoretical understanding with substantial predictive power will be attainable.

Three examples of outstanding issues concerning planetary interiors and the required theoretical effort will serve to illustrate this situation. The first concerns the layering of constituents within a planet (including the atmosphere and hydrosphere if any). Guided by appropriate laboratory data, we can model theoretically the partitioning of various starting constituents between coexisting phases. For example, one could predict the extent to which sulfur partitions downward in iron-rich phases rather than upward in volatile-rich fluids or phases in terrestrial bodies. This would produce a significant advance in our understanding of the overall compositions of Earth and the other terrestrial planets. It is also important to be able to calculate the fluid-dynamical processes whereby magma and gas find their ways from the interiors to the surfaces or near surfaces of planetary bodies. A synthesis of thermodynamical and fluid-dynamical considerations can lead to models that relate the observable characteristics of a planet to the expected initial distribution and mixture of planetary constituents.

A second example concerns the conditions required for active, large-scale plate tectonics. Why is the Earth apparently the only planet endowed with this phenomenon? Or is Venus also tectonically active? These questions can be approached by establishing the role of volatiles, particularly water, in the deformational properties of silicates and by exploring the role of phase transitions, such as basalt to eclogite, in providing some of the density contrasts required to drive motions. The physical principles developed through such modeling will also aid in understanding icy bodies such as Ganymede, where convection is likely.

The third example concerns the need to understand much better the origin of planetary magnetic fields, which should provide a unique and sensitive probe of planetary interiors. Generation of magnetic fields involves such fundamental quantities as internal sources of energy, convection patterns, and electrical conductivity. At present, whereas we apparently understand the basic physical principles involved in magnetic-field generation, we still cannot make quantitative predictions, for instance, to understand the specific implications of the strengths of planetary magnetic moments. This applies not only to other planets but also to Earth. A full model simulation of magnetic-field generation is far from achievable at present, but important advances in understanding remain to be made through investigation of idealized representations that explore various physical aspects of the process.

In the foregoing examples, as in numerous other cases, our understanding of the important processes is insufficient, and increased theoretical activity and numerical modeling, coupled with appropriate laboratory and observational efforts, are needed.

SURFACE PROCESSES

Both chemical and physical processes have shaped planetary surfaces, in determining the surface constituents and atmospheric interactions and the latter in determining the overall morphology. Our present understanding of the evolution of planetary surfaces, other than those of the Earth, the Moon, and, to a much smaller extent, Mars, is based largely on qualitative geomorphological studies. But as we venture to more exotic bodies with surface chemistries and atmospheres greatly different from those on Earth, traditional interpretations of surface photographs based on analogies to terrestrial landforms become suspect. The geologist cannot draw on terrestrial analogies when confronted by a hyperactive, sulfur-rich body such as Io, an ice-rich satellite such as Ganymede, or, to some extent, the enormous channel systems on Mars. For example, we do not know the long-term rigidity, or susceptibility to plastic flow, of ice sulfur under the relevant conditions; laboratory studies are needed to determine such properties before we can complete the descriptions of geologic processes in a physical framework. Continuing theoretical studies are vital in defining the needed laboratory investigations and for specifying the needed planetary observations and measurements.

Among surface modification processes, an improved understanding of impact phenomena on planets and satellites is especially needed. The hydrodynamics, shock heating, and energy partitioning during impact are poorly understood. Although a substantial effort exists in this area, supported by the Department of Defense and directed toward understanding explosion craters, a basic understanding of the dynamical properties of shocked material is still lacking. Without this theoretical and phenomenological framework, the scaling of experiments and Earth analogs to extraterrestrial impact structures has large uncertainties. Two problems of particular significance are energy partitioning and the generation of complex (multiring) structures. An understanding of energy partitioning is essential for calculation of the thermal state of accreting planets. For example, the extent to which impacts release volatiles from the solid material, thus creating a primordial atmosphere, is unknown. Similarly, the extent to which impact energy is retained to melt an accreting planet is not known. An understanding of complex impact structures may help to determine the compositional and thermal profiles of the outer layers of a planet. The existing large volume of data on crater morphologies is not adequately utilized because of our limited understanding of the physical processes. Computer simulations together with analytical and theoretical work on the flow characteristics of shocked material are needed.

It is necessary to develop a theoretical basis for extending existing geologic models to other planets. Conventional photogeologic interpretations can go astray when applied to surfaces subjected to unfamiliar processes or familiar processes operating in an unfamiliar dynamic regime. For example, what processes have generated the strange grooved terrain on Ganymede?

Another area where substantial data exist but more theoretical effort is required is in the modeling of internal and surface processes in small bodies. Extensive and varied meteorite data could provide important constraints on these processes, but the theoretical efforts have been very limited.

ATMOSPHERES

As with most areas of planetary science, the study of atmospheres is pursued on two levels: first, to describe and understand atmospheres in their current forms; second, to pick out those features that reveal information about atmospheric origins and evolution.

Meteorological studies fall firmly in the first category. The complexity of meteorological phenomena -- involving as they do fluid dynamics, thermodynamics, radiation physics, and chemistry, all taking place in turbulent, multiphase systems--makes it impractical to construct comprehensive models of the general circulation of an atmosphere. For Venus and Jupiter there are many theories, based on different simplifying assumptions, that have been proposed to explain their general circulations; but there are no adequate data for choosing among these theories. Thus an important role for theory in the meteorology of these atmospheres is to supply guidance as to what observations are needed and how missions should be designed. For example, two key investigations of Jupiter, needed to distinguish between various theories of the atmospheric circulation, are (1) the abundance of water, i.e., how important are latent heat transports and moist convection in shaping the large-scale circulations; and (2) how do the motions and the structure of the atmosphere differ in high and low latitudes, i.e., what is the relative importance of internal heating, solar differential heating, and rotation in shaping the large-scale circulations?

The chemistry and aeronomy of planetary atmospheres include many diverse aspects, each with its own theories. The study of spontaneous reaction and equilibration processes in tropospheres and on planetary surfaces is in the realms of thermochemistry and geochemical thermodynamics. Chemical physicists and aeronomers deal with the laboratory and planetological aspects of ultraviolet photolysis and gas-phase kinetics; cloud physics and chemistry and escape physics also play important roles. In each of these areas a coordinated program of theoretical and laboratory work is vital for continued progress. A particularly promising area for research is the theory related to laboratory simulation of planetary atmospheres and the synthesis of organic matter under conditions intended to simulate real solar-system processes. Such a theoretical effort must be based both on the theory of planetary atmospheres and on the chemical physics of the microscopic processes. Another area of great current interest is the attempt to understand, in detail, the behavior of ozone in Earth's stratosphere. This system has close analogies on Mars and Venus; a valid theory should properly describe all three systems.

A great variety of theories dealing with the origin of planetary atmospheres has been developed, but generally these are not soundly and directly based on fundamental principles. Such a theory should involve the initial volatile content of the planet-forming material, the sequence and rates of accretion of the various compounds, the thermal histories of the various layers, and, finally, mechanisms of transport to the surface of volatiles, their subsequent reactions, and their retention in the atmosphere. Many of these points are also aspects of the complex problem of the origin of the solar system. In spite of a lack of fundamental information, constraints on atmospheric origins can be inferred from the constituents of atmospheres such as isotopic and elemental composition of the rare gases. Theorists have a fundamental role there also in mission design in defining the precision to which the measurements of these rare gases must be made in order to allow meaningful interpretation and in pointing out possible pitfalls involving chemistry or dynamics that could frustrate a measurement by a particular experiment design.

COSMOGONY

Cosmogony, the theory of origin of the solar system, provides a framework into which the individual pieces of knowledge of the solar system must be fitted. Perhaps more importantly it concerns investigations of the fascinating problem of the origin of our environment and ultimately of ourselves. Cosmogonical models are constantly changing as they become increasingly constrained by the accumulation and evaluation of new data. Only when the mode of origin of the solar system is clear might star formation in general and the frequency of formation of accompanying planetary systems be understood.

Cosmogony is ripe for expanded theoretical activity since there are a large number of interesting and apparently tractable problems. All current theories of origin contain the assumption that the planets formed from a more or less flattened disk of gas and dust, with the Sun forming at the center of this disk through gravitational collapse. Unfortunately, the amount of material in the initial gravitational well, the relative timing of the formation of the Sun and that of the planetary bodies, the details of the accretion process, and many other facts and processes important to the theory are poorly known. Clouds of gas and dust with properties like those observed in the interstellar medium can be followed through gravitational collapse toward stable, rotating stars by three-dimensional numerical simulation, but the calculations have not yet included all the physical processes thought to be involved in the collapse to the high densities. There is a need to sustain this effort and expand the number of theorists undertaking the calculations.

Additional information about the physical state of the solar nebula can be obtained from research on meteorites, comets, and asteroids. Meteorites are of great interest because they are ancient probes of conditions and processes in the early solar system at the time of planet formation and are numerous, diverse, and readily available for study. Astronomical studies of the reflection spectra of asteroids have indicated that perhaps carbonaceous chondrites, the most volatile-rich meteorites, dominate the asteroid belt, while the metal- and FeO-bearing "ordinary" chondrites, which commonly fall on Earth, appear to be rare in the belt. These conclusions are based on comparison of reflection spectra of various meteorite classes with those of the asteroids, and no in situ observations to test these identifications have yet been carried out. Theoretical thermal evolution studies of small bodies also indicate that many asteroids and comets are so small that they may have survived virtually unaltered since the time of origin of the solar system. For this reason, theoretical interest in asteroid and comet missions and in the relationship between meteorite classes and particular asteroids has been high.

The composition of solar-system bodies depends on both the nebular chemical processes and the dynamics of accretion. Two processes of bulk accretion of the planets in the nebula have been proposed in cosmogonical theories, one involving gravitational instabilities in a massive nebula leading to giant gaseous protoplanets and the other involving gravitational instability only in a thin disk of dust at the central plane of a low-mass nebula leading first to kilometer-sized objects that subsequently slowly accrete to planet-sized objects. Both processes may have occurred in planet and satellite formation, but most of the consequences and details in either process remain uninvestigated. Special and perhaps unlikely circumstances might be necessary for one or the other process to succeed in making a terrestrial or a gaseous planet. Only by detailed theoretical investigations can the viability of either process be tested or perhaps yet another process be developed for accretion of planets.

After formation of the major solar-system bodies is nearly complete, or even while they are in the accretion phase, various evolutionary processes must have occurred before the system reached a relatively clean apparently stable configuration comparable with what we see today. Much nebular gas must be dispersed from the inner solar system and probably from the outer solar system as well. Asteroid-type debris must be swept away except for the remnant belt that we observe. Even the dispersion of the asteroidal orbit parameters within the main belt is a puzzle, since formation of the objects near the plane of the solar system would be preferred. The formation of the gaps within the belt is also not understood since sufficient collisions, coupled with the perturbations in the orbital eccentricities induced by the orbital resonances with Jupiter, to have created the gaps would also have damped the asteroids to nearly coplanar circular orbits.

Work on such evolutionary problems as these should be pursued to provide an understanding of the final transition from the formation process to a state resembling the present one. These final dynamic changes must have occurred regardless of the nature of the earlier condensation phase, but details such as the distribution of material to be swept away may depend on the type of condensation.

Altogether, there are many outstanding theoretical questions in the realm of cosmogony. Worker aimed at answering these questions would profit from increased and reliable funding, would contribute greatly to our knowledge, and would provide a firm basis for planning of future missions.

STATE OF SUPPORT FOR THEORY

The rapid growth and diversification of planetary science is largely a result of the vigorous program undertaken and supported by the NASA. Partly as a result of this historical fact, nearly all of the responsibility for support of this scientific endeavor has fallen to NASA, and little support is currently available from other agencies. The major proportion of the cost of carrying out planetary science and exploration is consumed by mission design, hardware, and operations, with only a small part of the support designated for basic theoretical and laboratory work. (We distinguish here between "data analysis" and basic theory aimed at understanding phenomena in terms of fundamental principles.) In the theoretical areas, which are the subject of this report, much of the present support appears to be targeted toward work deemed to satisfy preconceived notions of direct relevance to missions. However, most important discoveries and advances in theoretical research are unexpected and unpredictable.

One recent example of this was the prediction of the high internal temperature of Io due to tidal friction. The basic research that developed the analysis applied to Io was originally motivated by a desire to understand the role of tidal dissipation in the thermal history of the Earth's Moon. This earlier work excited little attention since tidal contributions to the Moon's total heat budget proved to be negligible. There was no connection with any mission at the time, but the research added to our store of knowledge of the solar system in general. However, subsequent application of the analysis to Io, just before the Voyager 1 discovery of active volcanoes on that body, catapulted the earlier work to a position of prominence and relevance as an explanation for a major mission discovery. This is an example where a small investment by NASA in non-mission-oriented theoretical research resulted in a significant scientific return and ultimately supported mission science in an important way. Similar examples are commonplace and abound through all areas of scientific research. It is essential to the health of space science that the basic, generalized research endeavors receive adequate support.

6 Computational Needs of Theorists

THE DIVERSITY OF COMPUTATIONAL NEEDS

The computational needs of a theorist in space sciences cannot be characterized easily. Some theorists are primarily oriented toward analytical work, using only paper and pencil. If they need to use a computer it is only on a small scale, to evaluate a few formulas perhaps. Only a few types of problems in the space sciences are amenable to this approach. The analytical theorist is drawn toward such problems primarily because they are tractable for him.

One type of such problem may be highly complex but governed by precisely known laws of physics or chemistry, which can be applied within fairly accurate approximations or in which the uncertainties can be clumped into parameters that are left for later determination. Investigations in celestial mechanics and planetary dynamics are often of this type. At the extreme lie problems that are attacked by back-of-the-envelope, order-of-magnitude methods, which primarily constitute the testing of the feasibility of new ideas.

Neither of these types of problems are likely to lead to a close confrontation between theory and observation in space sciences, yet both approaches are needed within the diverse theoretical community that contributes to our growing understanding of these subjects. The computational requirements of such theorists are usually minimal.

Other theorists may be closely involved in the analysis of space-derived data, in which case their computational needs are likely to be similar to the data-management needs of the experimenters. Since active collaborations between the theorists and the experimenters is frequently involved, it should not in general be too hard to accommodate the needs of the theorist within the same computational facilities that serve the data-handling needs of the Principal Investigator (PI) whose data are being used.

These computational facilities frequently consist of a minicomputer system located at the laboratory of the PI. Some theorists not engaged in data analysis may also have computational needs that can be met with stand-alone small computers, ranging from desktop microcomputers to minicomputer systems dedicated to extensive calculations. Such systems can be very cost-effective compared with the use of a local institutional mainframe computer, provided the annual cost of doing the desired computations on the mainframe is a substantial fraction of the purchase price of the smaller system. This threshold is quite easily passed by many investigators since increasingly larger fractions of the charges for mainframe time represent salaries of operators and systems personnel, while the cost of small stand-alone systems decreases visibly from year to year.

These computational costs can be quite modest parts of the expense of supporting small theoretical groups or of isolated theorists. However, it is necessary to draw attention to the fact that some problems in the space sciences require a massive computing effort for which the costs can be substantial. Generally these problems are those involving simulations of the physical and chemical behavior of systems in space. Such extensive computational efforts are essential both for understanding the fundamental behavior of the systems and for predicting the observable characteristics of the systems with sufficient accuracy to justify comparison with observations. We shall refer to a theorist engaged in such computations as a computational space scientist.

THE CHARACTER OF COMPUTATIONAL SPACE SCIENCE

An excellent description of the concerns of the computational space scientist can be deduced from a description of the computational physicist in the following quotation, which is taken from "Prospectus for Computational Physics," National Science Foundation, 1981:

> The computational physicist leads a triple life. First, and foremost, he or she must be fully conversant with conventional theoretical physics and the analytical techniques used by

theorists. Secondly, the computational physicist must have many of the practical skills of an experimentalist. Thirdly, the computational physicist faces the unique problems of computer programming.

Specialized physics knowledge must be combined with a general theoretical background in order to 1) formulate problems to be studied on the computer; 2) have the physical insight to guide the project through the myriad of decisions that must be made, and 3) be sure that the results have a real impact on the understanding of the problem being studied.

Equally important, tremendous analytical ability may be required to develop and understand the test cases and other tricks used to establish the correctness of a computational procedure (both the algorithm used and the computer program which realizes the algorithm). Since the solution space of discretized equations is very different from the solution space of partial differential equations, great care must be exercised in interpreting the solutions.

Furthermore, like an experimentalist, the computational physicist has an apparatus (the computer) whose recalcitrance must be overcome and whose idiosyncracies must be separated from the object being studied. The physicist must understand all sources of error in the calculations and design the code to keep all the errors from the approximations and numerical procedures under control. These include the problems of numerical instabilities, round-off errors and, in the most extreme cases, the possibility of random hardware errors. He or she must deal with the practical limitations of limited funding, pushing the capabilities of the apparatus to its very limits to enable carrying out computations on the forefront of physics research. Often considerable entrepreneurial skills are required to obtain the resources needed to carry out the research.

Finally, computer programming introduces problems. Many experimentalists also face this problem, but for the computational theorist the programming problems have led to special difficulties, including a great deal of misunderstanding and underestimation of the role and intellectual quality of computational physics. Computer programming and debugging is, in large part, a mind-dulling, menial task, where hours and days and weeks are spent making trivial changes in response to trivial errors, or figuring out how to format the output. Yet one must be able at any moment to apply the deepest analytical skills in order to understand an unexpected result or to track down a subtle bug. The computational physicist lives in perpetual terror that some unexpected combination of circumstances will cause the failure of a program in a real-life calculation despite the best efforts at debugging and testing the program. The computational physicist may need a detailed understanding of how the computer works, and the nature of the compiler compiling the program, in order to know how to test the program thoroughly and get it to run as fast as possible.

Underlying the practical problems of computer programming is the fundamental problem of readability of programs. An analytic theorist communicates his or her results by writing a paper in which the results of his work are derived. Another theorist can read this paper, rederive the results in it and use both the results and the derivation in further work. In contrast, computer programs, the means by which computational results are derived, are by their very nature unpublishable, and complex programs can only be read, if at all, at enormous cost in time and effort. Furthermore, reading a large program is not enough; to achieve the understanding of another theorist's program that is comparable to the understanding of another theorist's analytic result requires building a totally new program to carry out the same computation, running both programs, and tracking down all discrepancies.

PROBLEMS OF COMPUTATIONAL MODELING AND SIMULATION

The principal problem in computational simulation is obtaining an adequate representation of a physical system within the limitations of the capacity and capability of the computational hardware. Such a physical system is often represented by spreading throughout its volume a mesh of points at which the physical variables can be represented. The number of such points depends very much on the degree of symmetry that the system is assumed to have.

In a one-dimensional system 100 mesh points are typically required, although complexity sometimes drives the number over 1000. Such a system might involve spherical symmetry so that the dimension involved in the calculation would be the radial distance. Much modeling of stars and planets can be done this way on minicomputers, and evolutionary sequences of such models may take hours, days, or even a small number of weeks. On a mainframe computer such computations can usually be done about an order of magnitude faster than on a minicomputer, and sometimes they can be done 2 orders of magnitude faster on a computer such as the CDC 7600. Some theorists are content to work at minicomputer speeds, perhaps because they have no choice or because they value the interactive capabilities of the minicomputer. Other theorists feel that they cannot do their work properly without the faster feedback of the whole problem obtainable with the mainframes.

Most problems cannot be done with assumptions of spherical symmetry, except sometimes in an exploratory sense, so that two- or three-dimensional calculations become necessary for proper simulation. If each one of these dimensions must be divided into about 100 elements for proper calculation, then we start dealing with typically 10^4 mesh points for a two-dimensional calculation and 10^6 mesh points for a three-dimensional calculation. Calculations of hydrodynamical flows of interest in the space sciences are approaching these degrees of complexity. Some of the two-dimensional calculations have been done with minicomputers, but the three-dimensional calculations have generally required very fast mainframes and significantly less than 100 resolution elements per dimension.

Not all modeling and simulation computations can be described in precisely these terms, but there is usually some dimensional hierarchy involved in such problems, so that similar considerations apply.

From this it may be seen that the spatial description of a system is limited to rather low resolution. Attempts to obtain better resolution very quickly challenge the limits of computational capability. For example, to increase the spatial resolution by a factor of 2 would generally require increasing the number of mesh points in a three-dimensional problem by a factor of 8. The computation time may easily increase by an even larger factor, since it may be necessary to decrease the time interval between models in a model sequence. Since the pursuit of realism in simulation problems frequently demands the best attainable resolution, there are classes of theoretical problems that require massive amounts of computation.

MEETING THE COMPUTATIONAL NEEDS OF THEORISTS

It is important that NASA recognize that meeting the computational needs of theorists is an intrinsic part of the support for theory. This can sometimes be done through the inclusion of funds for use of the institution's central computational facility in theoretical grants or contracts. However, if the annual cost of such computing is within a factor of 2 or 3 of the purchase of suitable equipment that would allow such computations to be carried out by the theoretical group itself, then that alternative should be seriously considered.

In this respect it is important to take account of another difference between the theorist and the data-gathering scientist. The data analysis may require a stand-alone system, and it may be necessary to hire computational staff to take care of the chore of mounting and demounting a lot of magnetic tapes, as well as other data-management activities. Since the theorist does extensive calculations that rarely have a great deal of data input (usually called "number crunching"), such a computational staff person is usually not required. If the theorist is prepared to manage a computing system by himself, with the assistance of postdoctoral research associates and students, his computational costs can be quite small. If additional people must be hired, then the breakeven point at which his stand-alone system becomes cost-effective may be substantially raised.

The problems of support for large-scale computations are much more acute. In "Prospectus for Computational Physics" (National Science Foundation, 1981), it is estimated that direct expenditures on computing in theoretical physics through NSF grants amounts to about $400,000 per year but that the actual level of effort in such computations has been in excess of $10 million per year and is dropping owing to various fiscal problems. These computations are usually done by "moonlighting" on large mainframes, which means that their real costs exist but are charged to other efforts. This is not a healthy way to do science, and NASA should not rely on moonlighting as a way of carrying out large-scale calculations.

It is often suggested that NASA make time available on its large mainframe computers for theoretical computations by theorists it supports. In fact, if significant surplus time develops on such computers, NASA usually reponds by cutting back on the available hours for computation on those computers. Thus there is no practical way for the Office of Space Science and Applications to avoid paying the costs of large amounts of mainframe time in order to support the computational activities that are required for that kind of theoretical support. However, NASA should examine whether it can support these activities more economically by paying the marginal costs of keeping its mainframe computers operating for longer hours in order to accommodate theorists.

A special case is presented by the Numerical Aerodynamic Simulator, a large, fast computer that NASA hopes to build to form a kind of numerical wind tunnel that simulates the flow of air over the wings and bodies of aircraft but that will also be suitable for more general computations that can efficiently use parallel computation. Such a machine can make highly valuable contributions to intensive calculations in the space sciences, and we recommend that NASA make available some of its time for this purpose.

Another approach is also becoming feasible with the continued development of new hardware capabilities. Extensive calculations are now becoming possible through the use of array or attached processors that may operate in conjunction with a minicomputer or mainframe. These processors are reducing the unit costs of intensive computational efforts by substantial factors. We recommend that NASA provide funds to space-science groups with large computational needs to purchase and operate array processors attached to large minicomputers, where this would be more economical than supplying funding for computation on expensive mainframes.

7
Participation of Theorists in NASA Programs

DEVELOPMENT OF SPACE-SCIENCE STRATEGIES AND MISSION PLANNING

Spacecraft missions provide a unique means with which to carry out scientific investigations of otherwise inaccessible objects. Spacecraft orbiting the Earth or the Sun permit in situ measurements of the upper atmosphere as well as the magnetospheric and interplanetary plasmas, particles, and fields. Instruments placed above the Earth's atmosphere open otherwise inaccessible windows for observation of astronomical bodies ranging from the Sun to distant, exotic objects. Missions to deep space permit closeup scrutiny and investigation of the planets and exploration of the distant parts of the heliosphere. The motivation for undertaking these investigations is to elucidate the structure and composition of the world around us, to illuminate our understanding of natural phenomena, and to form the foundation of our knowledge of the origin and evolution of the solar system and the universe.

The opportunities for spacecraft missions are relatively infrequent; the missions demand a large dedication of time and resources. The choice of investigations and the structure and operation of the missions should ensure that they maintain a sharp focus on important scientific questions. This requires that missions should ensure that they maintain a sharp focus on important scientific questions. This requires that missions be designed on the basis of a sound picture of our present knowledge, understanding, and conceptions.

The framework of our understanding of the universe is embodied in our theoretical knowledge. The essential in-

Preceding page blank

fluence of this theoretical knowledge on spacecraft investigations has several parts.

The theoretical framework defines the significance of existing observations and of possible new observations. By tying together seemingly diverse phenomena, and by fitting them into a structure founded on simplifying ideas or on first principles, our theoretical knowledge points the way to new measurements and observations targeted most closely at carrying our understanding forward.

Our theoretical knowledge, based on earlier observations and measuremens, defines the precision necessary for new experiments to have a significant impact on our understanding. Subtle and diverse criteria must be used in setting measurement accuracies to be demanded of new missions. At the minimum, it is necessary that measurements be sufficient to differentiate among our major preconceptions and theories.

Altogether, just as in all areas of physical sciences, an effective and scientifically penetrating program of space-based investigations can only thrive in the presence of strong programs of theoretical study. In the earliest, exploratory phases of some investigations, the theoretical involvement is sometimes minimal; and in some areas of science, it is essential that some level of investigation of a purely exploratory character continue indefinitely. However, as individual disciplines evolve from their earliest discovery phases into the more advanced scientific stages that produce a conceptual understanding of objects and phenomena, the role that theory plays becomes prominent and essential. This evolution has occurred in solar and space physics and is now taking place in major areas of planetary science and astrophysics.

Two elements are necessary to provide an effective interaction between experimental space science and theory. The first is a strong overall program of theoretical research. This research should include a wide range of activities from phenomenological studies, sometimes in association with specific missions, to basic investigations of fundamental processes of general interest to cosmic science. A research program of this scope is necessary if the broad ramifications of ideas developed in space science are to be understood, if the detailed knowledge needed to support specific spacecraft missions is to be developed, and if the new ideas that will fuel future investigations are to be invented.

The second element in an effective interaction is the involvement of knowledgeable theorists in mission-planning activities at all levels from the formulation of long-term scientific strategies through the planning and operation of specific missions. As measurement capabilities become more sophisticated and as the questions that they address come closer to answering specific questions about the nature of phenomena, the design and execution of experiments require more detailed knowledge of theoretical ideas and predictions to isolate the most diagnostic measurements.

Participation by theorists in planning in astrophysics has been poor even at the strategy level, although recently it has improved slightly. For example, the Committee on Space Astronomy and Astrophysics of the Space Science Board has generally had 2 theorists out of 15 members and currently has 3. However, the NASA (in-house) Space Science Advisory Committee has no theorists in astrophysics. At the levels of working groups and mission planning, the situation is no better. The High Energy Mission Operations Working Group (HEAMOWG) has 0/16 theorists, the AXAF Science Working Group has 0/16 theorists, and similar figures hold for the Infrared/Optical and Ultraviolet Mission Operations Working Groups. As a consequence, a broad perspective on the issues that future missions can address is sometimes lacking, and theoretical input into the definition of individual missions is often haphazard.

A number of recent activities exhibit the necessary and fruitful trend toward a greater role in theory in the development of strategies and in mission planning and operations in the space sciences. These include the genesis and execution of the Pioneer Venus mission and the development of the Galileo mission in the planetary sciences, the planning of the International Solar Polar Mission in solar and space plasma physics, and the planning of the X-Ray Timing Explorer in astrophysics (which is described in Chapter 4).

In about 1968, a small group of theorists became concerned about the prospects for making significant progress in our understanding of Venus. A completely cloud-covered planet, it was proving intractable to the customary remote-sensing methods, even from flyby missions. The approach advocated was therefore an entry probe, instrumented to make direct measurements of structure, composition, cloud balance, and radiation fields -- an integrated set of parameters bearing on a wide range of basic understanding. Later, it became evident that three additional probes, less heavily instrumented, could be accommodated, and exploration of geographical diversity and wind fields was added. Suitable experiments were sought; one of these was developed into feasibility and successfully flown. In 1970, the Space Science Board sponsored a summer study that set the tone of the subsequent program, including the addition of an orbiter with heavy emphasis on aeronomy. This tradition of concentration on integrated scientific objectives, with appropriate theoretical input, has continued through all the subsequent phases of competition, selection, construction, operation, and extensions of the Pioneer Venus Orbiter mission. The same arguments applied to Jupiter have had great influence on the concept and execution of the Galileo mission, and many of the same people have been involved.

THE THEORIST'S ROLE IN FLIGHT MISSIONS

On the whole, there has recently been a significant participation of theorists in flight missions. Some future missions may lean more toward facility-type operations where a different organization may be appropriate. Our suggestions here may need modification with experience, although Guest Investigators can play a major role.

The classical roles for theoretical participation are the following:

1. Interdisciplinary Scientist (IDS) -- The IDS is selected by a procedure similar to that for an experiment, by evaluation of a proposal submitted in response to an Announcement of Opportunity. Funding is provided through the Project Office just as it is for an experiment.
2. Co-Investigator (Co-I) or experiment team member -- The Co-I is invited by a prospective Principal Investigator (PI) to participate in the writing of a proposal, the management of the experiment, and the data analysis. If the experiment is selected, funding for all the Co-I's is channeled through the PI.
3. Guest Investigator -- Sometime after launch (or encounter), it is usual to bring in a number of Guest Investigators to participate in the data analysis. Again, they are competitively chosen; funding may be through the Project Office or direct from NASA Headquarters.

Scientific guidance of an active mission rests with the Project Scientist, an employee of the field center that manages the mission, and the Science Steering Group (SSG; other names are also used, such as Project Science Group or Investigator Working Group). SSG membership includes all PI's and some or all IDS's. Normally, the chairman is the Project Scientist, but on Pioneer Venus (PV), it is an IDS; vice chairmen are the Project Scientist and another IDS. On both PV and Galileo, an SSG committee of half a dozen members participates actively in mission planning with several IDS members; and scientific working groups are usually chaired by IDS's.

IDS activities differ greatly before and after launch (for planetary missions substitute encounter for launch). Before launch the primary activities involve mission overview and detailed planning, through quarterly SSG meetings, meetings of the operational planning committee, and interactions between meetings with the Project Scientist, the Project Manager, and others. It is important that the SSG advise NASA and the project while trying to keep in view the overall goals of the mission. While outside bodies can and do help with this process, only the SSG has the detailed information that is most often necessary.

At the level of individual experiments, exactly similar functions should be performed by the Co-I's in advising the PI. The degree to which this actually happens is dependent on the management style of the particular PI, and on many experiments it seems to work well. It is also necessary to have adequate funds if the process is to succeed, and in the usual case of tight funding meetings of experiment teams are likely to be among the first to suffer.

Funding is required for IDS's and theorist Co-I's to perform their other natural function of doing scientific studies to prepare intellectually for analysis and interpretation of mission results. Again this money frequently disappears or is greatly reduced early in the life of many missions as more urgent demands appear. A recent and particularly flagrant case of cost-cutting involved the elimination of support for IDS's on the International Solar Polar Mission during one of its fiscal crises. Under such circumstances it is not surprising that many theorists turn to other activities until data start arriving. The tenacious few who remain active usually do so for the less tangible rewards of doing a good job and contributing to the success and effectiveness of a mission. These activities are therefore being subsidized by the theorists' institutions or their nonmission research grants, a questionable but all too common situation.

After launch (or encounter), most of these activities continue, although they are oriented entirely toward operations and sequencing since the hardware is otherwise unaccessible. Theorists participate in scientific analysis and publication. Many IDS's feel rather frozen out of this activity, which initially involves mainly the members of experiment teams. In the not infrequent case where the same individual plays both roles, the Co-I activity tends to dominate. Later, the truly interdisciplinary activity becomes both appropriate and feasible. Normally, there is agreement that IDS's, and often PI's as well, have access to many or all data sets for study purposes. When papers start being written, PI's whose data are involved are invited to become authors, and they may bring in members of their own teams. A still later phase is the organization of symposia, working-group papers, and books. IDS's are often active in such activities.

Funding is usually much closer to adequate for the postlaunch phase, though tightness is often evident. Other rewards are the usual ones for scientific activity: publication and presentation of papers, the respect of one's peers, and perhaps promotion.

THE ROLE OF THE Co-I

The advantages of theorists participating in hardware investigations are (1) input into the parameters that the instrument is designed to measure and (2) an early start in the use of inflight measurements.

Consider first the advantages of input to the instrument design. It is generally not possible to design the perfect instrument for a given mission that will provide definitive measurements for all conceivable phenomena. This restriction generally arises from limits to sensor technology and spacecraft logistics of mass, power, and telemetry. Hence, a compromise between desires and practicalities must be determined in order to set the observational and interpretive goals for the instrument proposal. Since the entire set of such goals for the successfully proposed instruments determines the detailed scientific purpose of a given mission, the direct participation of theorists in the instrumentation proposals directly affects the scope and direction of the mission. These benefits are not generally gained with separate independent proposals by theorists, interdisciplinary scientists, and others. As an example, in the case of the International Sun-Earth Explorer plasma instruments, an early compromise was necessary during the conceptual stage as to whether velocity and angular resolutions of ion and electron velocity distributions should be favored over temporal resolution. More precisely, the scope of the investigation was restricted by the telemetry rate assigned to the instrument. The decision was to favor detailed three-dimensional measurements of the velocity distribution and has resulted in current substantial progress in the understanding of plasma instabilities and convection in the Earth's magnetosphere.

After the conceptual and proposal stages of the mission, and before the period of analysis of inflight observations, theorist participation is minimal during the implementation phase. However, the interest of Co-I's may be greater at this stage relative to that of theorists attached to the mission in other roles.

The second substantial advantage of theorists, as Co-I's in an instrument investigation, is their ability to participate in the timely analyses of measurements in direct contact with the experimentalists. The familiarity that they have gained with the capabilities of the instrumentation and the direct line of communication with the experimenters seems to generate an atmosphere for more immediate analyses of the data.

In the past, it has often been financially expeditious to eliminate funds for theorists engaged in instrument investigations as Co-I's in order to minimize mission costs. The magnitude of the costs involved are relatively small and the gains are great since the attentions of the theorists are brought to bear immediately on the objectives and practicalities of a mission, as opposed to participating much more indirectly via general theoretical grants.

HISTORY OF THE IDS ARRANGEMENT

The pioneering mission in appointing IDS's was Atmosphere Explorer (AE), a set of three sequential spacecraft involved in a detailed study of the Earth's thermosphere and ionosphere. Three theorists were included in the Science Team as PI's. A relevant question in examining the role of theorists and the IDS in NASA missions is: "What events led to the inclusion of theorists on the AE team?" The answer to the question can be traced directly to the state of thermospheric aeronomy at the time the AE approach was conceived. In the mid-1960s thermospheric and ionospheric aeronomy had reached a point of stagnation. A great wealth of data had been acquired in the preceeding 10 years using rockets, Atmosphere Explorers A and B, and the Orbiting Geophysical Observatory series. Many new discoveries resulted from these missions; however, analysis of data was largely centered around the measurements made by a specific instrument by the PI and Co-I's.

These early missions constituted an exploratory phase in the history of the thermosphere, where most of the many important processes affecting the behavior of the thermosphere were identified, but quantitative evaluation of the integrated effect of these processes on the thermosphere was not possible or practical.

There were many reasons for the stagnation that was reached, not the least of which was the cumbersome way in which the acquisition and transmission of data to investigators was handled. Location of individual data sets at the sites of the PI's rendered coordinated problem solving too intractable a procedure to attract scientists to attempt broader-scope analysis.

It was realized by a small group of people at Goddard Space Flight Center (GSFC) and at NASA Headquarters that the modus operandi had to be drastically changed if significant new progress was to be made. Much effort had been expended in perfecting the individual instruments needed to study the multifarious processes that occur in the thermosphere. What was needed was the unification of a comprehensive group of such instruments into a single machine to realize the capability to measure simultaneously all the parameters needed to define the state of the thermosphere at any given location in space and any instant in time.

Many people, if not the majority, doubted the feasibility of the concept. It was not simply a question of hardware; the entire philosophy of doing upper-atmospheric science had to be changed. People had to be convinced that the time had come for individuals to cooperate with colleagues, organizers, and planners from NASA Headquarters and GSFC to pursue a unified attack on the thermosphere to its conclusion.

If the thermospheric system was to be treated as a whole, it was clear that the entire mode of doing science would have to involve a great deal of advance planning. A comprehensive understanding of the overall thermospheric systems was essential. PI's could not work entirely on their own, since there was now an obligation to produce data that would be optimized for the attainment of a greater goal. Hence, it was essential to have scientists with a broad perspective of the problems to be addressed. Guidance could be provided in terms of what parameters would be most useful and what ranges and accuracy would be required to address various problems. A fundamental aspect of the unified or team approach was the need for ready accessability of all data by all team members, which was achieved via a central computer at GSFC, which maintained a large fraction of the data on-line in a form that could be accessed via remote terminal links.

Historically, the time was ripe for this unified approach to solar-terrestrial problems to begin with the thermosphere. Clearly such an approach would not function successfully in an entirely exploratory space mission, since quantitative planning would not be possible. Nevertheless, clear and useful roles have emerged for the IDS on missions that include a significant exploratory aspect. Any planetary mission, for example, involves tour planning that demands science input from many areas to optimize trade-offs.

Following AE, many far-sighted people in the scientific field recognized the value of the AE approach. There was a growing realization during the 1970s that solar-terrestrial physics had progressed to a level of understanding similar to that of the thermosphere in the mid-1960s and that if the solar-terrestrial system were to be treated as a whole, the same basic philosophy would apply. Naturally, data would be gathered by many vastly different instruments on widely differing missions. This diversity of requirements with accompanying potential trade-offs, more than ever, indicated the need for the role of theorists, or IDS's, in future missions.

The success of the AE approach to the thermosphere led a small group of scientists familiar with the AE system to investigate a similar approach for the exploration of Venus, which evolved into Pioneer Venus (PV). They suggested that the AE approach be followed particularly for the Orbiter, which was the first aeronomy mission to another planet. Eleven IDS's were subsequently chosen for the two Pioneer Venus missions. IDS's have been deeply involved in all phases of the mission, and their relationship with PI's has generally been excellent.

The Space Science Board endorsed the PV model in Strategy for Exploration of the Inner Planets: 1977-1987 (Space Science Board, 1978b). Although this recommendation was directed specifically to planetary missions, it has been taken up by NASA and has been widely applied to solar-terrestrial and astrophysics missions as well. The following quote is from that publication:

> INTERDISCIPLINARY SCIENTIST STATEMENT
>
> In considering the role of the scientific community for the purposes of studying and analyzing data from a deep-space mission, it is the view of COMPLEX [Committee on Planetary and Lunar Exploration] that during the postflight phase "members of a more general scientific community as well as the experiment team should be involved in the study and data analysis" (Report on Space Science 1975, National Academy of Sciences, Washington, D.C., 1976).
>
> Extending this point of view, COMPLEX has discussed the desirability of a broader participation by interdisciplinary scientists during a mission. As a result of extensive discussions, COMPLEX unanimously agrees that every proposed planetary mission should follow the lead of Atmosphere Explorer and Pioneer Venus in having interdisciplinary scientists appointed to the mission Science Steering Group. These people should have the status of Principal Investigators and an equal vote or a voice on the Science Steering Group. Their most obvious role during and after the mission is to organize cooperative investigations using data from more than one experiment. They can have, however, a different and perhaps even more valuable part during the planning and early operational phases. Here they can assist project scientists to maintain balance among the experimenters and group of experiments

and ensure that the scientific return is optimized. They should bring a broad scientific view to the mission and see that priorities are maintained in their proper order. In the case of choosing alternate mission profiles, the interdisciplinary scientists can aid the scientific decision-making process without a personal commitment to the use of a particular instrument. The presence of interdisciplinary scientists on a steering group should greatly aid the project scientist in keeping the major mission objectives in proper perspective.

We concur with the procedure used on Atmosphere Explorer and Pioneer Venus to select interdisciplinary scientists on the basis of their response to an announcement of flight opportunity (AFO) and endorse it as standard practice in the issuance of AFO's for all future missions.

With the advent of AE and PV and with Galileo in an advanced stage of development, we have sufficient data to review and assess the actual role that the IDS has played in NASA missions to date.

A verbal poll of the AE, PV, and Galileo communities yielded an unanimous and resounding YES to the question: "Has the IDS proved to be a valuable asset to NASA missions?" Reasons for this response are given below. In the case of PV, the IDS's assumed an active role in mission planning. Unlike AE, where the Project Scientist chaired the Science Team, the chairman of the PV SSG was an IDS elected by the SSG. The IDS's provided valuable independent guidance in mission planning. The Project Scientist is always a member of a NASA center and as such closely tied to the Project Office. The IDS's are generally quite independent and have exhibited no tendency to be unduly influenced by the project. Rather, the IDS community has injected much original thinking into the system, which has stimulated the project to respond in a positive way. Minor reports were received of lack of appreciation on the part of some IDS's of the limitations imposed by vailable resources. However, there is general agreement that the IDS's contributions have had a positive effect on mission planning.

Although PI's have generally filled the role of the IDS in the past, the PI is preoccupied with the welfare of his own instrument. His highest priority is to achieve what is best for his investigation, i.e., more weight, more power, more money, and getting it built.

The IDS on the other hand is generally an individual unburdened with hardware responsibility. Although there are cases of some IDS's who have not participated in mission planning, the majority have served in several valuable roles. For example, in the case of PV and AE, IDS's prepared review articles of outstanding problems to be attacked. The IDS's were lead authors in those instances and shouldered much of the work. On Galileo, the IDS's have voiced their views at Project Steering Group meetings from the planning phases, and this has proved valuable. Their contributions have brought perspective to the discussions involving trade-offs with regard to the tour design, particularly in making value judgments that are based on overall goals to be attained. Such guidance has been valuable to the Project Manager, who has to make difficult decisions between science on the one hand and risk or other factors on the other. The IDS's from different disciplines can help in making sound judgments when resource limitations force choices between modifying one instrument or omitting another. Their independence permits a more unbiased opinion as to what is of greatest scientific value. A primary asset is that the IDS body constitutes a source of information familiar with the ins and outs of the project from the start.

Because the IDS is frequently an individual of stature in the field, they have served to represent the interest of the mission on national committees and to Congress. They also have played an important role in keeping the National Research Council informed in their analysis of missions. Generally the IDS's have demonstrated the willingness to work for the Project and to compile necessary reports, which would require time the experimental PI's are often unable to give.

In the data-analysis phase, the IDS's have successfully carried out their primary responsibilities, namely, the integrated analysis of data taken from many instruments, with the development of new theoretical models. Although guest investigators have played valuable roles, the IDS's are more familiar with the orbital details of the mission, the status and quality of data, and of the various instruments.

The cost of an IDS is small compared with the overall cost of the mission. As such, NASA received a very high return for each dollar invested in the IDS, yet many IDS's did not have adequate funds to attend SSG meetings. The IDS program within the International Solar Polar Mission was one of the first items to be eliminated in a budget cutback.

People polled were unanimous in the opinon that IDS funding was inadequate and that their contributions to a mission are significant.

PLANNING FOR THE UPPER ATMOSPHERE RESEARCH SATELLITE

The Upper Atmosphere Research Satellite (UARS) program was planned and is being conducted to maximize the contributions of the theorist. In this sense, the term "theorist" really indicates an individual whose participation in the program is not connected to the design and fabrication of an instrument. The UARS Science Working Group (SWG) was established by the Solar-Terrestrial Office of the Office of Space Sciences, NASA, in October 1977 to develop a satellite program to conduct research on the chemistry, energetics, and dynamics of the Earth's upper atmosphere. Approximately half of the SWG were theorists in the aforementioned sense. This was considered appropriate since one of the principal virtues of this satellite system was that it would allow the study of the interaction of dynamic, chemical, and energetic input processes by simultaneous measurements on a single spacecraft. It was recognized that the interaction among physical processes is encountered more frequently in the construction of theoretical models of the upper atmosphere than in the design and construction of specific instruments.

The final report of the UARS SWG stated the following: "It is expected that the overall direction of scientific activities within the various UARS missions will be directed and coordinated through a Science Team composed of the experimental and theoretical Principal Investigators." The responsibilities of these two types of PI's was set forth as follows:

> The Experimental P.I.'s, and their Co-Investigators, will have the usual responsibility for developing a particular instrument and conducting analyses of their observations to yield quantities

of geophysical interest. In addition, it is expected that the experimental teams will participate in extensive data analysis and interpretation using relevant theory. Another category of Principal Investigator, and associated Co-Investigator, will include theoreticians with expertise spanning a broad range of interests relevant to the upper atmosphere, as well as the interpretation of experimental data. On the one side, there is a need for theoreticians to be involved in the conversion of radiance data into accurate geophysical information. On the other, geophysically oriented theoreticians are needed to conduct extensive diagnostic studies, to develop new theory, and to verify theory by comparing the results of complex simulation models with the actual behavior and structure of the atmosphere.

The preliminary selection of investigations was announced in the Spring of 1980 and included 16 experimental PI's and 10 theoretical PI's with about twice as many experiments selected as will eventually fly. Thus, the UARS program has been conceived and carried out, so far, in a manner that ensures significant involvement of theorists in the program. This is crucial to the success of the UARS, or indeed to any program that is built around the detailed analysis of physical processes rather than being exploratory in nature. As more and more NASA satellite programs seek detailed understanding, it will be ever more important to assure significant involvement of theorists in all phases of these missions.

PROBLEM AREAS

Once NASA has chosen the members of an SSC, it leaves it to that group and the Project Office to define its own roles and relationships. This degree of autonomy is generally good but carries some problems. The PI holds far more cards, more resources of both money and manpower, and control of information, data, and Co-I funding. Some SSG's have adopted a set of "Rules of the Road," taking as a starting point those agreed on by the AE team. Such rules have seldom needed to be invoked; the experience of working together for several years has made this unnecessary.

There are still problems; the IDS who does not contribute until the data start flowing or the PI who is unable to release the data or unwilling to do so until everything can be guaranteed are examples. In many cases peer pressure can be brought to bear; administrative pressure is available but may not be effective because the cooperation of the individual is essential, or because real problems exist that take time and resources to solve.

RULES OF THE ROAD FOR AE INVESTIGATORS AND CO-INVESTIGATORS

1. Investigations may be sponsored only by Investigators (AE Aeronomy Team members).

2. Data will be placed in the public domain two years after acquisition, when those not having an association with AE may have access to the data.

3. Investigators may not "stake-out" specific areas precluding other Investigators from working in that area.

4. Any Investigator whose data are being used in an investigation has the right to be included in the authorship of a resulting publication. It is the responsibility of the prime investigator to solicit the participation of the investigator (which may be declined) whose data are being used, during the formative stages of an effort or paper.

5. There is no AE Aeronomy Team editorial policy with regard to paper form or publication medium.

6. Investigators, co-investigators, and associates have access to all Team data and publication thereof, but the Investigator "in charge" is responsible to the Team for proper control of the data used in accord with 4 above.

7. Proposed and tentative investigations (topics) will be sent to all Team members and included in the PSM file listing unless some Team member makes his objections known to the Project Scientist.

8. Independent scientists, not controlled by a Team member, who supply data may also have access to data in accord with the policy set forth

in the "Opportunities Document." Thus, outside personnel can participate with an Investigator if (a) letters of cooperation in response to a Team invitation limiting and specifying data availability and exchange are exchanged (copies to Team members) and (b) Team approval is obtained prior to problem work initiation. Cooperation with personnel outside the Aeronomy Team must be based on their providing suitable correlative data to the Team and must be channelled through an Investigator. Investigator interest, participation, and responsibility are mandatory.

9. Investigators can release their own data to anyone they wish, but not the data of other Investigators without their consent.

AE Team
June 1974

Insufficient funding is a problem that has been discussed above; it is so fundamental that it is mentioned again here. If NASA regards itself as a mission-oriented agency, it would seem logical for missions to be fully self-supporting; support of IDS's and Co-I's is seldom adequate. An additional problem for the Co-I is total dependence on the good will of the PI, even if the experiment as a whole is adequately funded.

The fundamental resource in managing or monitoring an activity is information. This is why it is important to hold frequent meetings of SSG's and experiment teams. A critical decision or an oversight by a manager or PI can be detected only if SSG or team members know what is going on. In practice, there are many more checks on a mission than on an experiment, and it is up to each PI to see that the team is in fact kept informed. The PI arrangement, with responsibility and authority vested in one person, is time-tested and effective; but one defect is that there is no mechanism to enforce this flow of information. In principle, the team members should take the initiative if the flow is inadequate, but in practice it is difficult for them to know.

8
General Recommendations

From the discussion in the preceding chapters we conclude that theoretical research of high quality is being carried out in all the major physically oriented areas of space science: astrophysics, planetary sciences, and solar and space physics. This research contributes to the space program in a variety of ways in addition to its value as science. The effectiveness of the theoretical effort in many areas of NASA activity is limited by the small number of its funded practitioners not by the availability of challenging and important problems. Some subdisciplines suffer in having only a single active group, a situation in which healthy criticism becomes scarce or absent. A substantial expansion of the relative level of effort in theoretical space science within the total research program is justified by the quality of current work and its importance to NASA missions.

NASA needs theorists to support the development of strategies for space science and the planning of space missions, as well as in other activities requiring a broad viewpoint. In many cases it is using a resource that it did not develop or does not help to support. In a more programmatic vein, a resource that is not cultivated may shrivel and become unavailable when most needed. Theoretical studies independent of particular experimental or observational programs are valuable in all branches of physical science.

SOLAR AND SPACE PHYSICS

We commend NASA for establishing support for an independent theoretical research program in solar-system plasma physics. This progam of grants is a balanced one, with efforts in solar, solar wind, magnetospheric, and, to a lesser extent, atmospheric physics. It emphasizes strengthening the role of basic plasma physics in solar-terrestrial physics. To this end, many new plasma physicists, who never before contributed to space physics, have been funded, and support was provided for numerical modeling, one of the principal terrestrial programs recommended by many review panels. Many younger theorists, who it is hoped will contribute to space physics for many years, were given important responsibilities.

We also note with approval that the recent NASA definition of the proposed OPEN magnetospheric mission included, for the first time, theoretical objectives as part of the prime mission. Should OPEN be funded, we hope and expect that a newly strengthened theoretical community will play an important role in designing and carrying out its mission.

Since many new theorists, and many younger ones, have been brought into NASA's new solar-terrestrial theory program, it is essential that it be protected against inflation, so that those involved in it will have time to bring their programs to fruition. At the same time, new theoretical objectives not included in the present program will inevitably emerge. Therefore, continuing review by NASA and the Space Science Board will be needed to assure the optimum evolution of the Solar Terrestrial Theory program. Several years will be required to judge whether this program encouraged theory "to play a central role in the planned development of the field."

It is important that NASA's support of theory through its existing Supporting Research and Technology programs be maintained. Otherwise, the goal of the new program to strengthen the existing terrestrial community substantially will be compromised.

Increasing the theoretical sophistication of solar-terrestrial physics goes hand in hand with enhancing the quality of its experimental data. Recognizing this, the Committee on Solar and Space Physics recommended (Space Science Board, 1980):

> Moreover, theory and quantitative modeling should guide its [solar-terrestrial physics'] entire information chain--data acquisition, reduction, dissemination, correlation, storage, and retrieval--to a higher level of sophistication, to

provide prompt availability of coordinated data of diverse origins.

Solar-terrestrial physics has important ties with laboratory plasma physics, planetary physics, and astrophysics. Because these subjects are linked at the conceptual level, and because their experimental techniques are very different, it is clear that theory must lead the way toward realization of the benefits of cross-fertilization of idea and technique. NASA currently supports research involving plasma theory in its astrophysics, planetary physics, and solar-terrestrial physics programs. We urge that NASA take a more unified viewpoint toward all of these efforts, since they form parts of the broad discipline of plasma physics. We would hope that each grouping of the plasma community would respond to NASA's broader outlook by widening its research objectives. In this way, for example, universities will find the case even more compelling than at present to support basic research in plasma physics "outside the laboratory." To begin this process, we recommend support for interdisciplinary workshops and laboratory experiments that can illuminate the behavior of space and astrophysical plasmas.

ASTROPHYSICS

In order for theory to realize its important and proper role in astrophysics, two elements must exist: (1) a strong and independent program of theoretical studies and (2) participation and involvement of theorists at all levels of mission planning.

The first element requires a range of theoretical activities, from the analysis of data through basic and fundamental theoretical studies not associated with particular missions or mission objectives. Such a range of activity is necessary for a healthy and vibrant theoretical community, capable of supporting the present directions of space-science investigation but also able to suppport new and unexpected directions.

The participation of theorists is needed at all levels of mission planning, from the development of space-science strategies to the definition of individual missions through mission operations. Only by such involvement can the best use be made of the infrequent opportunities for space-science missions and can rapid absorption and digestion of the resulting data occur. Both of the above two elements are increasing in importance as major areas in astrophysics requiring spacecraft observations become (necessarily) problem, rather than discovery, oriented.

Theoretical astrophysicists have often been minimally involved in the planning of past space experiments. Similarly, theoretical studies that specifically address how best to utilize future space data have been rare. As a result, the level of mutual awareness on the part of both theorists and observers is low. Theorists often fail to recognize the relevence of forthcoming space observations to their research. Experimentalists often are unaware of recent theoretical advances in understanding the phenomena they study. In the past, the low level of theoretical involvement did not seriously compromise the quality of scientific results obtained from space missions. Each new wavelength range studied led to new and unexpected discoveries. Such a strategy was acceptable, and perhaps even healthy, in the early exploratory stage of observations.

However, this approach has obvious limitations that have already been felt in many areas. As we enter an era of problem-oriented observations of phenomena at all wavelengths, observational strategies that are based on the joint consensus of both theorists and observers must be evolved. Such strategies will balance what is technically feasible with what is most useful in gaining a deeper understanding of the cosmos.

The above lack of communication is due at least in part to the fact that theoretical astrophysics has been funded almost exclusively by the National Science Foundation, an agency that has no direct responsibility for space missions. It is also due in part to the fact that NASA has not been active in encouraging broadly based theoretical discussions of its mission goals and their results.

Increased theoretical involvement and activity are also needed to assure that the unique and rich data derived from future space missions are utilized fully. Currently, the number of theorists doing relevent calculations and model building is small, and their level of support is low. As a result, our ability to understand astronomical phenomena and to make sense of data from space missions is lagging seriously behind the rate at which new data are gathered.

In order to strengthen a resource that is critical to the proper utilization of future space-astronomy missions we recommend that NASA establish an independent research program to support talented theorists in astrophysics whose research interests include areas where space-derived data do or will exist. NASA should take a long-range view of the resulting research and judge it on the quality of its contributions to new understanding and utilization of space astronomy. Long-term investigations of particular phenomena of interest to space observations should be emphasized.

This program might be used to support (1) theoretical studies by qualified individuals whose research interacts with planned or existing space missions, (2) block grants to theoretical groups, (3) the establishment of theoretical groups at institutions where major observational programs (especially in space) are being carried out, (4) travel grants to facilitate theoretical participation in mission planning and instrument design, and (5) workshops to evolve space mission objectives and disseminate the results of space observations.

While recognizing that meaningful and important theoretical input has been sought in the planning of past spaceflights, we recommend an increased participation of theorists in the planning and design of future space missions. This should occur through several means, including (1) increased membership of theorists on committees and panels charged with defining space-science strategies and mission planning, (2) more theorists as guest and co-investigators on space experiments (although not necessarily deriving major, continuing, research funding from such investigatorships), (3) NASA-sponsored workshops on mission characteristics and goals that involve the participation of the general theoretical community before the mission has been defined, and (4) inclusion of theoretical analysis and theorists as a major component of institutions such as the Space Telescope Institute, which will be centers for receiving incoming space data and for its archival storage.

PLANETARY SCIENCES

In Chapter 5, we gave examples where increased theoretical research is needed to carry forward our understanding of the solar system and to provide the basis for future missions and observations. It is important also to realize that much research must precede definition of these missions beyond the initial exploration stage. Only in this way can we ensure that spacecraft investigations will be targeted to address the most significant scientific questions.

Currently, basic theoretical research that is not connected with a specific mission is supported by several individual program offices within NASA. In the subject of planetary atmospheres some 35 percent of the papers at a recent Principal Investigators conference were theoretical in nature, and for planetary geophysics and geochemistry 31 percent of a recent work report dealt with theory. In other areas, including lunar sample analysis, meteorite research, planetary geology, and planetary astronomy, the fraction of theoretical effort is much lower. About 16 percent of the research and analysis funds in planetary sciences has been going for theoretical research. Thus this theoretical effort is weaker than the other areas of support, which include experiment, observation, and data organization and analysis.

As a consequence, important problems are receiving little or no attention because too few scientists are working in these fields; the overall NASA mission of understanding in a fundamental way the nature of the solar system and ultimately its origin and evolution will thereby be delayed. Theorists should be attracted from other relevant fields and young scientists given some promise of job stability so they will be inclined to enter the field. This is needed in order for NASA to realize the goals stated in its charter. The uninterrupted involvement and commitment of theorists must be maintained by sustained support. Otherwise, many of the best scientists will turn to other more reliable sources of support in other disciplines. The lack of career stability has produced an erosion of graduate student enrollment -- both in quantity and quality. There is a danger that the competent active and experienced core of theoretical planetary scientists will erode to the point where it cannot support the continuing NASA planetary program.

In some areas, the experimental data are limited and increased support for theoretical investigations should be accompanied by a simultaneous effort to improve the knowledge of values and relationships of physical parameters and material properties needed for modeling. In many cases the data required are detailed laboratory measurements. Theorists attempting to model planetary interiors, for example, are currently limited by the available laboratory and observational data. This unsatisfactory situation can be overcome by a balanced program in which new theoretical efforts are initiated in conjunction with expanded laboratory efforts to develop the appropriate data. Some of these data, such as the behavior of planetary materials at high pressures, are becoming available because of improved experimental techniques, and it is essential that such programs receive adequate support.

Existing support for basic theoretical investigations in planetary science is insufficient, both to establish and to maintain the increase in basic knowledge needed for planetary mission planning and to attack with vigor many of the areas in which important advances in understanding can be made. There is a need for greater diversity of theoretical effort that is not directly tied to current or planned missions but that is aimed at increasing our understanding of important solar-system processes through basic, generalized research.

A necessary response to these needs is the infusion of additional theorists into planetary science, since the present number is too small to make the required impact on many outstanding problems. An environment of sustained support must be created in which young theorists find this field attractive and see a future in the form of career opportunity and stability.

Part of the difficulty with career opportunities in planetary sciences is that this multidiscipline is seldom included within the departmental structures of the universities, and career academic positions in planetary sciences are scarce. Theorists working in the areas of planetary sciences are therefore frequently required to work in a nonacademic environment or to work in one of the disciplinary academic departments that contribute to multidisciplinary research in planetary sciences. We recommend that NASA provide stable, long-term funding for new planetary-science theorists in related university departments, in NASA centers, and possibly in other organizations.

COMPUTING

It is important that the general support for theorists recommended here should include funding for their computing needs. Because of the extensive use of numerical simulation, these computing needs are greater in the space sciences than in many other areas of science. The support should include provision of computing systems to groups of theorists ranging from small systems to large minicomputers with attached array processors, where these systems would provide cost-effective alternatives to the outside purchase of computing services. Other possibly cost-effective alternatives involve the provision of computing on large NASA mainframes, including the proposed Numerical Aerodynamic Simulator.

FLIGHT MISSIONS

Since 1970, theorists have played increasingly active and important roles in flight missions, especially those that benefit from an interdisciplinary approach. These roles include the interdisciplinary scientist, the co-investigator, and the guest investigator. This trend is valuable and is to be encouraged. Steps should be taken, however, to assure adequate funding of theorists on flight missions with a high enough priority so that their funding is not unduly cut in financial emergencies. Such cuts have become a serious problem recently. If funding through a project office cannot meet this recommendation, other arrangements should be considered. A possible model is the recent practice of budgeting data-analysis funds as an item distinct from the support of individual missions.

References

SPACE SCIENCE BOARD

Report on Space Science 1975. National Academy of Sciences, Washington, D.C., 1976

Space Plasma Physics: The Study of Solar System Plasmas. Volume 1: Reports of the Study Committee and Advocacy Panels, S. A. Colgate, Chairman. National Academy of Sciences, Washington, D.C., 1978a.

Strategy for Exploration of the Inner Planets: 1977-1987. National Academy of Sciences, Washington, D.C., 1978b.

A Strategy for Space Astronomy and Astrophysics for the 1980's, Committee on Space Astronomy and Astrophysics. National Academy of Sciences, Washington, D.C., 1979.

Solar-System Space Physics in the 1980's: A Research Strategy, Committee on Solar and Space Physics. National Academy of Sciences, Washington, D.C., 1980.

Strategy for Space Research in Gravitational Physics in the 1980's, Committee on Gravitational Physics. National Academy Press, Washington, D.C., 1981.

ASTRONOMY SURVEY COMMITTEE

Astronomy and Astrophysics for the 1980's, Volume 2: Reports of the Panels. National Academy Press, Washington, D.C., 1983.